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" Nec aranearum sane textus ideo melior quia ex se fila gignunt, nec noster vilior quia ex alienis libamus ut-apes." Just. Lips. *Polit.* lib. i. cap. 1. Not.

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"Meditationis est perscrutari occulta; contemplationis est admirari perspicua . . . . . Admiratio generat quæstionem, quæstio investigationem, investigatio inventionem."—*Hugo de S. Victore.*

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—" Cur spirent venti, cur terra dehiscat,
Cur mare turgescat, pelago cur tantus amaror,
Cur caput obscura Phœbus ferrugine condat,
Quid toties diros cogat flagrare cometas;
Quid pariat nubes, veniant cur fulmina cœlo,
Quo micet igne Iris, superos quis conciat orbes
Tam vario motu."

J. B. Pinelli ad Mazonium.

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PLATES.

ERRATA.

Page 352, line 25, *instead of* like a jelly in the fact *read* like a jelly from the fact.

— 357, — 31, *instead of* composition of fibrous mass *read* composition of the fibrous mass.

— 358, *instead of*

$$B' = \frac{B+A'}{A},$$

$$C' = \frac{B+A'}{A}$$

read

$$B' = \frac{B \times A'}{A},$$

$$C' = \frac{B \times A'}{A}.$$

— 359, in the Table, *instead of* calculated in 5·74 albumen *read* calculated for 5·74 albumen.

— 362, line 5, *instead of* between the albuminous *read* between these theoretically albuminous.

I. *What determines Molecular Motion?—the Fundamental Problem of Nature.* By JAMES CROLL, *of the Geological Survey of Scotland*[*].

IT is an opinion which is daily gaining ground that at some future time, perhaps not far distant, all the purely physical sciences will be brought under a few general laws and principles. However wide and diversified physical phenomena may seem at first sight, and however great and radical the apparent distinction between the several sciences, yet to the eye of the thoughtful physicist, who sees deeper into the subject, they begin to appear as but the varied modifications of a few common principles. For example, Heat, Electricity, and Magnetism are in their ordinary phenomena very unlike each other; yet modern investigation has shown that they are mutually convertible. Heat can be converted into Electricity, and Electricity into Magnetism. Magnetism can be converted into Electricity, and Electricity into Heat. This indicates that these corresponding *sciences* are not radically distinct, that their phenomena have a common origin, that in each we have the same force manifested under different forms.

To arrive at unity among the facts of nature has ever been, and ever will be, the aim of physical investigation. We try to induce a unity amongst the multifarious facts of the senses by bringing as many of them under a certain conception as will be rationally connected by it. But we soon find that we must have a higher unity; and we endeavour to reduce the number of

[*] Communicated by the Author.

our conceptions by finding one of a higher order—and so on, ever trying to reach the highest unity, the most general conception possible.

The point, however, which more immediately claims our special attention at present is this :—As the physical sciences proceed in their generalization, they advance more and more towards Molecular Physics.   We may illustrate our meaning by examples without number from any of the sciences.   In Electricity, for example, all the ordinary questions—such as how a Leyden jar becomes charged, or how the electricities are supposed to be decomposed on a conductor, or by what means one body charged positively will act upon another body charged negatively—were formerly considered to be answered quite satisfactorily without making any reference whatever to the molecular condition of the bodies under the electric influence.   But when we come to inquire more deeply into what is meant by induction—what that peculiar condition is which constitutes the charged jar, and the nature of that hidden change which takes place on the conductor while what we call its electricities are being decomposed,—we begin to find that we are entering upon deep and difficult questions regarding the hidden operations taking place among the molecules or the atoms of the electrified body.   Electricity was formerly supposed to be a fluid substance altogether distinct from the body in which it manifested itself; and, of course, inquiry was directed towards this hypothetical substance, and not to the molecular condition of the body.   But the grounds for believing in the existence of this hypothetical fluid are fast disappearing ; and electricity is now generally believed to be a condition or motion of the molecules or the atoms of the electrified body itself.   Similarly, heat at one time was considered to be a substance to which the name Caloric was applied.   It is now, however, proved to be not a substance, but a particular mode of motion of the atoms of the heated body.   The same is proved to be the case in regard to Light ; and Magnetism, which was formerly explained by means of hypothetical fluids, is now believed to consist also in a particular molecular condition of the magnetic body ; whilst Chemistry is fast becoming a question regarding the dynamical actions of the atoms of the combining substances.

In Physical Astronomy questions regarding the constitution of the sun, the fixed stars, and nebulæ are being determined by Molecular Physics ; and even the direction and velocity of their motions are now resolved by the same method.   Physical inquiry in every direction is converging towards Molecular Physics, is resolving itself into questions regarding the dynamical action of the ultimate particles of matter.

To determine (1) the constitution of the ultimate atoms and molecules of matter, what they really are, and (2) their behaviour (the laws of their motions), are two great problems of Molecular Physics towards which all physical investigation is tending. These are the two important problems which first present themselves for solution ; but neither of them, as we shall see, is the grand and fundamental problem.

On a former occasion I referred to some considerations bearing on the first of these two problems *. I shall now briefly refer to the second, which, in consequence of its more immediate relation to the ultimate question of scientific inquiry, is of much more importance than the first.

The second problem, we have seen, refers not to the nature of the molecule, but to its motions. Now in regard to all physical change or motion, no matter what the nature of that change or motion may be, there are at the very outset two fundamental questions which suggest themselves :—(1) What produces the change—causes motion ? (2) What determines or directs it ?

In regard to the first question, there is no diversity of opinion. All agree that what produces change or causes motion is Force. The second question, however, viz. what determines or directs the motion, is not so easily answered. This question is not only the more difficult of the two, but also by far the more important.

All physicists agree that what is called Physical Law is just the expression of the manner in which forces act in the production of their effects, or " the paths along which they travel to their particular results," as Mr. Lewes expresses it †. In the production of all physical phenomena we have therefore two distinct elements, viz. force, and the way or manner in which force acts —force, and the paths along which it travels, so to speak—or, in other words still, Force and the Laws of Force.

One of the most important results of modern physical inquiry has been to show that the various phenomena of Light, Heat, Electricity, &c. are but different modifications in the action of the same forces. When the forces take one path, we have Light ; taking another path, we have Heat ; another produces Electricity, and so on. Now it will be observed that the fundamental question is not, what is the particular force in action, or upon what does its exertion depend, but rather what is it that causes the force to act in the particular manner in which it does act ? In other words, what determines the paths along which it acts ? Physical phenomena are produced in general by the motion of the molecules or of the atoms of bodies ; now the great question is not simply what produces the

* Philosophical Magazine for December 1867.
† Comte's Philosophy of the Sciences. By G. H. Lewes. Section V.

motion, but what produces the particular kind of motion ? It is not what gives *existence* to the motion, but what *determines its direction* ? This is evident, because the particular phenomenon, regarding which our inquiries are concerned, does not directly depend upon the mere existence of the motion, but upon its special direction or determination. The same exertion of force which produces one phenomenon would probably produce any other phenomenon, were determination in the proper direction given to it. It is the *determination* of the force which accounts for the particular phenomenon ; the mere *exertion* of force may be supposed to be the same in all phenomena. We are therefore led to the following proposition, viz. :—

(1) *The* Production *of Motion and the* Determination *of Motion are absolutely and essentially different.*

The radical and essential distinction between motion and the determination of motion could not possibly have escaped the observation of physicists ; but the important bearing that this distinction has on physical inquiry has certainly been overlooked.

In physics we have been accustomed to attribute every thing to force ; force, at least, has always been regarded as the all-important element. This, however, is a mistake ; for, as we shall see, far more depends upon the *determination* of force than upon its *existence*, and therefore, *unless force be determined by force, the most important element in physical causation is a something different from force.* And this holds equally true whether our inquiries relate to the inorganic or to the organic world.

In the production of organic forms from the simplest up to the most special and complex in the vegetable and animal kingdom two things require to be accounted for, viz. (1) the motion of the matter of which they are composed, and (2) its *disposition* or *arrangement* with reference to time and space. The particles which are to compose the organism must not only move, but move with a particular determination in regard to time and space. If a molecule has to be placed in any particular place of an organism, it must move in the particular direction in *space* which will lead to that place, and stop at the particular moment of *time* when it reaches it.

Motion is not only produced, but it is produced in a particular manner and under particular conditions or determinations in regard to time and space and other circumstances. In other words, not only must something produce the motion, but something must determine it also. The causing of, or giving mere existence to the motion, I have called the *Production* of the motion. The causing of it to happen in the particular manner in which it does, rather than in some other manner, I have called

the *Determination* of the motion. It must be evident to every one who will consider the matter that these two things are radically distinct. And they are not only radically distinct, but must be separately accounted for. To account for the mere existence of motion, does not account for its happening in one way rather than in some other. It is quite true that the one cannot be produced without the other; we cannot determine motion unless there is motion to be determined; we cannot determine that which has got no existence; neither, on the other hand, can we produce motion without at the same time giving it some particular determination in regard to time, place, or other circumstance. But, although the one cannot be produced without the other, yet they are the result of different agencies; and to assign a sufficient cause for the one does not in the least degree satisfy the mind as to the presence of the other. To account for the motion of a ball does not account for why it moves, say, east rather than west or in any other possible direction. A force, it is true, cannot act without at the same time acting in some particular way, nor move a body without moving it in some particular direction; but to account for the one does not satisfy the mind in regard to the other. The explosion of the powder within a gun is a sufficient cause for the motion of the ball, but the explosion of the powder is not to the mind a sufficient cause why the ball moves east rather than west, or in any other direction.

The grand and fundamental question then is, What is it that determines or directs the action of the forces concerned in the production of molecular change? The question therefore regards not Law but Cause, unless we use the term law in an improper sense. Law in physics is not an agent or force, it is simply the *process* or mode of operation—not the force, but the path along which the force acts. I know no clearer definition of law than that given by Mr. Lewes, to which I have already referred.

Suppose the subject of our inquiry to be the origin of a crystal, the leaf of a tree, or any other special form, organic or inorganic. We inquire, first, what is it that moves the particles while the crystal or the leaf is being built up? We refer the motion to a *force*, and feel satisfied with the explanation. But force or energy accounts for the mere motion of the particles. We inquire, next, what are the particular paths taken by the moving particles? In what manner or way do they move to their positions? In other words, what are the *laws* of their motions? But even if we knew this and could answer both of these questions, we should not be satisfied. We must not only know the paths taken by the particles, but must be able also to explain why the paths are taken. If we knew not simply the path, but the

cause of the path being taken, we should then understand the law of the moving particles; we should then perceive how the selection of the particular path was a necessary result of something within our knowledge.   A prodigious number of physical phenomena are perceived to follow as necessary consequences from Newton's grand law, that bodies tend toward each other with a force varying inversely as the square of the distance and directly as the mass of the bodies.   But we should reach a higher unity and obtain a deeper insight into nature did we know not merely the empirical fact that bodies do so, but the cause why they do so. It is this which incites in the rational physicist the desire to find out the cause of gravity.

But be all this as it may, whether it be Cause or Law, that is the thing which we are really in search of, every one will admit that the problem of deepest interest is, what causes the molecules and particles of living nature to arrange themselves into organic forms?   The problem is not what moves the particles, but what determines or directs the motion—or, in other words, what is the cause of the determination of motion?

What, then, determines molecular motion in organic nature? What determines and directs the action of the forces concerned in the production of specific forms in the inorganic and organic world?   Is it a Force?   This leads us to the second proposition, viz.

(2) *The action of a Force cannot be* Determined *by a Force, nor can Motion be* Determined *by Motion.*

That the action of a force cannot be determined by the action of a force is demonstrable thus.   If the action of a force is determined by an act, then this determining act must itself have been determined by a preceding act; and this preceding act by another, and so on in like manner to infinity.   This is evident; for if the act which determines the action of the force exist at all, it must exist in time and space, and must have a determinate existence in reference to time and space, and if so, something must have given it that determinate relation.   If it be replied that it was a prior act which determined this determining act, then that prior act in order to give the determining act the proper determination must itself have been already properly determined; and the question again recurs, what gave this prior act the proper determination?   If the determination was given by an act still prior, that act must itself have been properly determined; and if so, then there must have been another act preceding which gave it the proper determination, and so in the like manner to infinity.   The reason of all this is perfectly obvious. When we account for the determination of an act by assigning

an act, we account for it by means of a something which requires itself to be accounted for in a similar manner. To make the matter still more plain, suppose that an effort or exertion A of force has been produced. It has not only been produced, but it has been produced at a particular time and in a particular place, and in reference to some particular thing; that is to say, the effort not only got existence, but a particular determined existence. There are, therefore, two things which require to be accounted for :—(1) the mere exertion of the force—its simple production; and (2) why this particular effort A was made instead of B, or C, or any other possible effort, and why it happened at this particular time and place rather than at some other. That is, we have to account (1) for its *production*, (2) for its *determination*. Now what we are inquiring after at present, is not what *produced* this effort or exertion of power A, but what *determined* it; that is, what caused this particular effort A to happen rather than B, or C, or any other, or to happen rather than not to happen, or to happen in the particular manner in which it did happen rather than in some other manner. It must be evident that whatever this cause may have been, it was not an *exertion of force.* For let us suppose that the effort A was determined as to the manner of its happening by an effort B, then this effort B itself requires to be accounted for in a similar manner; for if the effort B happened, it must have happened after a particular determined manner; and if so, we must ask what determined the effort B— what caused it to happen in that particular manner ? And if we say it was by an effort C, we involve ourselves in a similar manner, and so *ad infinitum.*

But even supposing we were to maintain that the determination of the effort A was caused by effort beyond effort *ad infinitum,* it would not help the matter the least; for on looking at the question more closely, we find that it is not the determining act or effort B which determines the effort A, but the *determination* of the determining effort B. We find that if this determining effort or exertion of force B had not been itself particularly determined as to the manner of its happening, the effort A determined by it would not have happened as it did; so its happening in the manner in which it did happen depended upon the effort C which determined the determining effort B. And again, on looking more closely we find that it did not depend upon this effort C either, but upon the *determination* of this effort C. And even supposing we were in this manner to go back to infinity, still it would not be the antecedent act which determined the consequent act, but the *determination* of the antecedent act which determined it; so that, after all, by no possible means can we conceive determination to be the result of an act, or ex-

ertion of force.   Hence, be the cause of the determination whatever it may, it cannot possibly be an act or exertion of force.

In a similar manner we can prove that motion cannot be determined by motion.   Motion will produce motion, but motion cannot determine motion.   A ball A in motion will produce motion in a ball B, but the motion of the ball A will not determine the motion of the ball B, either in regard to direction or to the times of its happening.   The particular direction taken by the ball B is not due to the motion of A, but to the particular direction in which A is moving at the moment in which it produced motion in B; so that the direction taken by B must be referred not to the motion of A, but to that something, whatever it may be, which causes A to move in the particular direction in which it moves.   In other words, the determinate direction taken by B is not due to the motion of A, but to the *direction* of the motion of A.   In like manner it can be proved that the direction taken by A is not due to the motion of some other body (say C), but to the *direction* of that moving body C.

In a similar way we can prove that the particular time at which B begins to move is not due to the motion of the striking body A, but to the *particular time* at which the body A strikes B.

As attraction and cohesion play important parts in all physical phenomena, it will be as well to satisfy ourselves that the determination of motion cannot be referred to these.

Suppose two particles, A and B, are at some distance from each other, A being to the east of B.   Let them move toward each other under the influence of their mutual attraction, A to the west and B to the east.   Their motions are due to attraction, but not the *direction* of their motions.   Attraction is the reason why A moves, but not the reason why it moves west rather than east.   This is not due to attraction, but to the fact that B *happened to be on the west side* of A when the motion took place.   So the direction taken must not be referred to attraction, but to that something, whatever it may have been, which was the cause why B happened to be on the west side of A.

In regard to attraction under every form, the direction taken by moving particles is due to the prearrangement of those particles in regard to time and space.   A difference in prearrangement would necessarily produce a corresponding difference in the directions taken by the particles.

Or let us take another very simple case of motion, viz. the motion of a planet in a circular orbit.   Most people at first would be inclined to refer the circular path taken by the planet to the action of forces.   They would say that the form of the orbit is produced by the action of the two forces, centrifugal and centripetal.   But if we examine the matter properly, we shall

find that it is produced by no such cause. The mere motion of the planet is indeed due to these forces; but the path taken by the moving planet cannot be referred to forces at all. The circular path of the planet is not the result of forces, but of the *way* in which the forces are applied. The planet moves in virtue of centrifugal and centripetal force; but it moves in a circular path because these two forces are so adjusted to each other that the one tends continually to pull the planet in a straight line towards a point called the centre of the orbit, while the other tends continually to make it move at right angles to that line. But it may be replied that this particular adjustment of the two forces can be referred to a force; for, according to the nebular hypothesis, the motion of the planets in their orbits results as a consequence from the condensation by gravitation of the nebulous matter out of which the system was formed. It is no doubt in all probability true that the entire energy of planetary motion was derived originally from gravitation. The *vis viva* or mere motion of the planets is the result of gravitation; but the paths of the planets—the direction taken by them—cannot be referred to gravitation. The motion of rotation induced in the nebulous mass—the thing which originally determined not only the separate existence of the planets but the form of their orbits—cannot be referred to gravitation, but to the *way* or *manner* in which gravitation acted on the condensing mass. In the condensation of the nebulous mass, if the particles had moved directly towards the centre of gravity of the mass, no motion of rotation would have been induced, and consequently neither planets nor orbits would have ever existed. The paths, therefore, of planets are not due to the condensation of the mass by gravitation, but to the way in which gravitation acted upon the mass. Had the mass condensed without rotating, all that is now the *vis viva* or energy of the planetary motion would have appeared as heat.

Hence that which determined the existence of the planets as separate bodies, which determined that so much of the energy of gravitation should be converted into *vis viva* of planetary motion instead of into heat, and which determined that the planets should not only move but move in circular or elliptical orbits, was not gravitation, but that something (whatever it may have been) which caused the nebulous mass to condense in such a way as to produce a motion of rotation. Our solar system is therefore the result not of force merely, but also of the *determination* of force. When the matter is properly analyzed, we find that all that belongs to force is merely the *vis viva* or energy that the system possesses. The plan of its existence, the arrangements of its separate parts, and in fact everything else that can be predicated of it but mere energy, are the result of that

something which determined or directed the action of the forces concerned in its existence.

The vague and indefinite idea that the arrangement of the molecules of matter into crystalline and organic forms is due to the action of forces, appears to be implied in such terms in common use as " structural forces," " formative forces," " crystal-building force," &c.   It is supposed that if our mental powers were enlarged or strengthened so that we could perceive every thing connected with the forces operating in nature, we should then be able to explain the process by which the organic forms of nature are built up.   This, however, is evidently a mistake. Though our acquaintance with the forces of nature were absolutely perfect, the question as to how particles or molecules arrange themselves into organic forms would probably still remain as deep a mystery as ever, unless we knew something more than force.

The mystery is not what are the forces which move the particles, but what is it that guides and directs the action of the forces so that they move each particle in the particular manner and direction required.   Force gives motion to the particles; but we are not concerned about the cause of the motion, but about what *directs* that motion.   It is replied that motion cannot possibly take place without its being in some particular direction.   But this does not prove that the two things are the same.   It only proves that they are inseparably connected. Neither does it prove that the same thing which is assigned as a cause for the one will serve as a cause for the other.   Two effects may be inseparably conjoined and yet result from different causes.   But the two effects under our consideration are not inseparably connected.   It is true that a molecule cannot move without moving in some particular direction; and thus far the two effects are inseparable; but a molecule may move without moving in the *proper* direction.   These molecular forces cannot act without acting in some particular way; but they may act without acting in the proper way.   Now what is it that determines that they shall act in the proper way?   When a molecule is to be moved, there is an infinite number of directions in which force may be conceived to move it.   But out of the infinite number of different paths, what is it that directs the force to select the right path?

Is it asserted that force is self-directing?   This is simply getting into confusion again.   What conceivable idea can be attached to a self-directing force?   Is force a something which not only acts but determines for itself how and when it shall act? In what conceivable way can force direct its own path?   A molecule has to be moved into its proper place in an organic form;

a force gives motion to the molecule; but out of the infinite number of possible directions in which the molecule may be moved the force moves it in the right direction. What is that something which thus guides the force? The force guides itself, it is replied. Be it so; but in what way does the force direct or guide itself? What is the nature of that something in virtue of which the force directs its actions? Is it supposed that that something belonging to the force which thus guides and directs its action is itself a force? Does the force direct itself by means of a force? if so, then we are back to our old absurdity of a force determining a force. And if this directing something is not a force, what is it? But if this something is not a force, it follows that there is something else to be known than mere force before we can penetrate the mystery of nature.

The simple truth is, in attempting to account for the *determination* of motion by referring it to a *force*, we are attempting an absolute impossibility. The *production* of motion and the *determination* of motion are two things absolutely different in their essential nature. Force produces motion; but it is as impossible that force can determine motion as that two can be equal to three, or that a thing can be and not be at the same time. The necessity is as absolute in the one case as in the other.

If any one imagines that he can conceive motion as being directed or determined by a force, he will find, on subjecting his thoughts to a proper analysis, that the determination is not due to the force which he imagines, but is due to the *direction* in which his imagined force exerts itself. The determination results not from his imagined force, but from the *way* in which his force acts.

We have been accustomed to speak of organic forms being built up particle by particle by the play of molecular forces; and probably most of those who know little about science imagine that scientific men attach some clear and definite idea to such a statement. They naturally conclude that the scientific physicist understands in some way or other how, and in what way, these forces may be conceived to build up the structure; and they no doubt would feel surprised were they told, what in reality is the plain truth, that the physicist who uses those terms knows just as little about how the play of forces can build up an organic structure as he does himself. The idea has gained a footing that the thing is done in some way or other by forces; and although in the mean time we cannot comprehend the manner in which it is done, yet we imagine that at some future day all will be plain.

But if it were possible that the *shape* or *form* of any thing in nature could be the product of a force, surely we ought by this time to be able to imagine or conceive how the thing may thus

be done in some special case or other. But I have never yet seen the attempt made. Take the simplest of all special forms, viz. the crystal. In what possible way can we conceive the crystalline form to be the product of forces? If there be any form in nature that can be accounted for by means of the "play of forces," we should expect it to be the crystalline. But let us see whether the theory generally held regarding the mode in which the crystal is formed gives any support to the opinion that *form* can be the product of force.

The particles or molecules of which the crystals are composed are supposed to be of a certain definite shape ; and it is supposed that they attract one another at certain definite points or along certain definite lines. The molecules therefore cohere together in a fixed and definite manner; and thus a figure in the form of a crystal is the result. This may or it may not be the true explanation. But it will be at once perceived that all that force does in the matter in such a case is simply to move or draw the molecules and hold them together in the crystalline state. The crystalline *form* is therefore not due to the force, but to the original *shape* of the constituent molecules, together with the fact that they attract one another at definite points. Consequently that which determined the form of the crystal is not the forces, but that something, be it what it may, which is the cause why the molecules have such a shape, and why their attraction is confined to the definite points on the surfaces of the molecules. All that the molecular forces do in the case under consideration is simply to pull ; the form of the crystal is due not to the pull, but to that something which gives to the constituent particles their specific shape and directs the forces where and how to pull.

In short, let us in imagination form any conceivable hypothesis as to how an organic or a crystalline figure may be produced by a force, and we shall always find, on subjecting our hypothesis to a logical analysis, that the form and the arrangement of the parts do not result from force, but from something else. We need not in any way feel surprised at this ; for we are in reality, as has been already shown, attempting to do what is in itself absolutely impossible. The production of form or the arrangement of parts by a force is what never is, was, or can be effected.

As the distinction between the production of motion and its determination, or between the production of an act and its determination, is absolute, it must hold equally true in the mental world as in the physical. For example, it is just as impossible to conceive the Will being determined by an act, as to conceive the motion of the cannon-ball being determined by the explosion of the powder. It is difficult to say whether in physics or in metaphysics the distinction is of most importance. It would be

foreign to our present purpose to enter into the consideration of this distinction in relation to mental phenomena. This I have done at considerable length in a work on the fundamental principles of Theism, published several years ago*, to which I beg to refer any who may be interested in this aspect of the subject.

What is the cause† of determination? What is that something which determines the energies of the universe and guides the motion of the material particles? That this is the all-important question, whether as regards life-theories, theism, or evolution, will be still more obvious after considering the next and third proposition to which we are led, viz. :—

(3) *All the Energies and Forces of nature are probably the same, and differ only in regard to their modes of operation.*

This proposition follows as a consequence from the principle of the Conservation of Energy, viz. that the sum total of the energies in nature remains constant, the amount neither being increased nor diminished.

Suppose now that two substances (say, oxygen and hydrogen) combine chemically. Heat is evolved as a consequence. The energy in the form of heat is derived from the energy in the form of chemical combination. The energy which disappears in chemical combination reappears as heat. We have first chemical energy and then heat; not first annihilation of chemical energy and then creation of heat. The energy which now appears as heat is the self-same energy which previously existed as chemical energy. The energy has only changed its form, and nothing more.

Suppose the heat to be applied to move a machine and to perform mechanical work. What appears as mechanical energy (mechanical motion) disappears as heat; and the energy stored up potentially as work performed, say, in the raising of a weight, is the self-same energy which previously existed as chemical energy and then as heat. The same holds true whatever may be the number of the transformations. Chemical combination will produce an electric current; the electric current will produce magnetism; and the magnetism will produce motion in a machine; and the machine will generate heat or perform work.

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* The Philosophy of Theism. Ward and Co., 1857.

† The term Cause is by some writers arbitrarily restricted to force or energy. It is assumed that every effect must be the result of an exertion of power. Of course I do not here use the term in this narrow and restricted sense. To affirm that force, and only force, is cause, and that every event, every thing which comes to pass must be produced by an exertion of power, is to beg the whole matter in dispute; for the very point I have been endeavouring to prove is, that force, or the exertion of power, cannot possibly be the cause of the determination of motion.

Here we have the energy assuming in succession five or six different forms. While the particles are combining we call the energy chemical; when the electric current is produced we designate the energy electrical; when magnetism is produced we designate it magnetic; and when the machine is in motion we call it mechanical, and so forth. It is the same energy under all these various forms. The only difference between chemical, electric, magnetic, and heat energy is merely in the mode of operation. The difference lies therefore not in the force or energy itself, but in its determinations. If we regard heat, light, electricity, magnetism, chemical action, &c. as but different modes of motion, as they in reality probably are, then the difference between chemical action and heat, or between heat and electricity, or between electricity and magnetism, or between magnetism and mechanical motion, &c., depends wholly on the *cause of the determination of motion.* The difference does not lie in the mere exertion of force, but in the *way* or *manner* in which force is exerted.

We may now consider the bearing which these propositions relating to the determination of motion have on some of the questions which at the present time are agitating scientific minds.

Let us consider, first, their bearing on theories regarding "vital force" and the mystery of life.

Theories of Life.

To a large extent the discussions and diversity of opinion which at present prevail in reference to the mystery of life and the distinction between the organic and the inorganic world take their rise from confusion of ideas regarding the difference between the cause of motion and the cause of the determination of motion. The various theories may be divided into two classes,— the advocates of the one class maintaining that all the phenomena of life, all the changes which take place in organic nature are the result of purely chemical and physical agencies; while the other party maintain that there must be something more than the ordinary chemical and physical forces at work—in short, that life and organic nature imply the action of a force altogether different from those which belong to the domain of chemistry and physics, and to which the name of "*Vital Force*" has been applied.

Both parties appear to be to a certain extent right, and both to a certain extent wrong. Let us begin with the consideration of the Vital-Force theory.

In what respect, then, is vital force supposed to differ from other forces? Does the difference exist in the force itself, or in the mode of its operation? Is vital force the same as the che-

mical and physical forces, although only differently determined?
Suppose that all life on the globe, both animal and vegetable,
were to be destroyed and vital force to disappear completely, would
the total amount of energy on the globe be diminished? Would
the vital force which disappeared reappear as chemical or as phy-
sical force? or would there be a destruction of force? If the
former be supposed, then there is no difference between vital
and the other forces of nature further than in the mode of ope-
ration. Vital force would in this case simply be the ordinary
forces of nature transformed, or, in other words, the ordinary
forces differently determined. But if we suppose vital force in
itself to be different from other forces irrespective of its mode of
operation, and that when it ceases to be vital force it does not
become ordinary chemical or physical force, but disappears alto-
gether, then the destruction of vital force would involve a viola-
tion of the principle of the conservation of energy. If we do
not admit a transformation of vital energy, we must assume that
when a plant or an animal decays and dies, so many foot-pounds
of energy existing in the molecules become extinct. And, on
the other hand, when a plant or an animal increases from the
embryo state to maturity, so many foot-pounds of energy come
into existence.

Such a view of vital force as this would be diametrically op-
posed to the modern science of energy, and wholly untenable.
Evidently the vital energies of the plant and animal are derived
from the chemical affinities of the food and nutriment which
they receive. Vital force is chemical force transformed. The
same remark holds true of the mechanical and other physical
energies of the body. The energy by which the arm is raised or
by which the heart beats is derived from the food. Animal heat
is derived from chemical combination.

So far as all this is concerned, the advocates of the phy-
sical theory of life are evidently correct. But are they warranted
in affirming, as they do, that all the energies of plants and ani-
mals are either chemical or physical? Whether such an affir-
mation be correct, depends entirely on the idea which may be
attached to the terms chemical and physical. If what is meant
be that all the energies in organic nature have had a chemical or
physical origin, and that there is no energy in nature which has
not at one time existed either as chemical or as physical energy,
then no one acquainted with the science of energy would for a
moment question the correctness of such a conclusion. In this
case what is termed vital energy would simply be transformed
chemical or transformed physical energy. It would differ from
the energies in operation in the chemical and physical worlds
only so far as the mode of operation is concerned: the forces

are the same ; only they act differently. But if this be what is meant, then assuredly every force in nature is either chemical or physical. But this would be using the terms chemical and physical energies in a peculiar and unusual sense.

We are accustomed to name forces and energies according to their mode of operation. Oxygen and hydrogen unite under the force of their affinities ; and we designate the energy of the combining substances " chemical energy." After combination the energy assumes another form ; and we call it " heat." The heat is applied to the thermo-electric pile and becomes transformed ; and we call the energy under the new form by the name " electricity " or " electric current." The electricity is applied to the electromagnetic machine ; and the energy assumes another form, to which the name " magnetism " is applied. The magnetism propels a machine and performs mechanical work ; and we then call the energy " mechanical energy." These various names are applied to the various modes of operation of the selfsame energy. Chemical energy, for example, in the case under consideration, differs from heat only in the mode of its operation.

We have also been accustomed to group heat, light, electricity, magnetism, gravity, cohesion, &c., under one class, to which we apply the general term physical, or physical energy, in contradistinction to chemical energy. We thus distinguish chemical energy from all the other forms, because we conceive it to be concerned with the combinations and motions of the atoms or elements of substances, whereas the other class deals with the molecules and masses of matter.

Now when the advocates of the physical theory of life affirm that every energy in organic nature is either chemical or physical, they certainly do not mean to include under the term physical every form of energy which does not, like chemistry, deal with the elementary substances ; for if this were their meaning, it would be simply a truism to say that all energy is either chemical or physical. By physical energy they undoubtedly mean the ordinary and known forms of energy manifested in the inorganic world, to which we give the various specific names of attraction, repulsion, light, heat, electricity, magnetism, and so forth. But here we now approach the real question at issue, viz. are these forms of energy along with chemical energy sufficient to account for the phenomena of life and organic nature ?

Chemistry and physics are insufficient, because they do not account for the *objective idea in nature.*

Whatever maybe one's opinions regarding the doctrine of Final Causes and the evidence of design in nature, all must admit the existence of the objective idea in nature. We see everywhere

not only exquisite order and arrangement in the structure of plants and animals, but a unity of plan pervading the whole. We see, in endless complexity, beauty, and simplicity, the most perfect adaptation of means to ends. The advocates of the physical theory are at least bound to show how it is probable that this exquisite arrangement and unity of plan could have been produced by means of chemical and physical agencies.

Let us briefly consider what really has to be explained and accounted for. Take, say, the leaf of a tree. The leaf is not moulded by some external agency into its particular shape, but is built up molecule by molecule. The form and structure of the leaf is the result of the arrangement and disposition of the particles of which it is composed. The thing to be accounted for is not what moves the molecules or particles in its formation, but what guides, directs, or determines the motion of these particles. The leaf could not be formed did not each particle move in the right direction and stop at the proper time and at the proper place. Each molecule occupies its own special position in the leaf; consequently no two molecules in moving to their positions can take the same path. What, then, determines the particular path for each molecule? or rather, what determines the motion of each molecule along its particular path? The mere motion of the molecules is produced by force; but what directs or determines this force to move each particle along its special path? But the mystery is deeper still. Not only are the paths of the molecules different, but they must all be adjusted in relation to one another; for it is to the proper adjustment of the paths that the form of the leaf is due. In other words, the motion of each molecule must be determined according to the objective idea of the leaf.

But the whole tree is built up of molecules, as well as the leaf. The molecules which form the branch must be differently determined from the molecules forming the leaves; and each molecule of the branch must take a path different from all the other molecules of the branch; but the motions of all the molecules must be determined according to the objective idea of the branch. What holds true of one branch holds true of all the other branches; and what holds true of the branches holds equally true of the trunk, and of the roots, and of the whole tree. Each particle must be determined not only in relation to the objective idea of the particular leaf or the particular branch to which it belongs, but in relation to the objective idea of the tree. In the formation of the tree each molecule must move along its special path, but the paths must be so adjusted to one another that a tree shall be the result. But this is not all; the molecules must move and adjust themselves in relation to the

idea of a tree of a special kind. The molecules forming, say, an oak tree, must move in relation to one another in a different way from those forming a beech tree or a pine. But however diversified may be the motions of the molecules in the different species of trees, yet, notwithstanding, all must move in relation to the general idea of a tree. And what holds true of trees holds equally true of every form of plant-life on the globe. And what holds true of the vegetable kingdom holds equally true of the animal kingdom. Each plant and each animal has not only its own particular form, but it has the form of the species to which it belongs—and not only this, but the form of the genus to which the species belongs—and not only the form of the genus, but the form of the family, order, class, and kingdom to which the genus belongs.

Taking, therefore, the entire molecular movements going on in the organic world, animal and vegetable, we may classify the determination of these movements into kingdoms, classes, orders, families, genera, and species, in the same way as we classify the plants and animals which are the result of these determinations of molecular motion. This is obvious, because this order and unity which the botanist and the comparative anatomist find pervading nature, owes its existence to the order and unity which exists amongst the determinations of molecular movements. A plant or an animal of a particular species and a particular class exists simply because the molecules of which it is formed had their motions determined according to the objective idea of a plant or of an animal (as the case may be) of the particular species and class. This is not asserting any thing hypothetical; it is simply stating what actually takes place; for to say that the molecules of which a tree, for example, is composed must have had their motions determined according to the objective idea of a tree, is just the same thing as saying that the molecules of which a tree is composed must have had their motions determined in relation to an object of the figure of a tree. In nature we have a unity of plan pervading the endless diversity that everywhere prevails, simply because the endless and the almost infinite diversity of molecular movements take place according to a unity of plan.

In nature we have a group of molecular movements corresponding to the objective idea of each particular object that is being formed. In objects of the same species the groups of molecular movements have a specific resemblance to one another, while in the formation of all objects of the same genus there is a generic resemblance between the groups of molecular movements. In the formation of objects of the same family we have a still higher unity, comprehending a still greater number of

groups of molecular movements. We go on in like manner till we reach a unity which comprehends under it all the groups of molecular movements occurring in the vegetable or in the animal kingdom. The unity which pervades the endless diversity of molecular movements must be as perfect as the unity which we find to pervade the endless diversity of organic forms. In fact the two are inseparable, because the unity which exists amongst organic forms is the *effect* of the unity which exists amongst molecular movements. It is because these molecular movements are determined according to a unity of plan, that their effects (viz. organic objects) have a unity of form. It is the particular determinations of the movements of the molecules that give the particular form to the tree.

It may, however, be noticed that a thing may be the result of the determination of molecular motion, although it may not be so directly. For example, that which determines the arrangement of the buds on a twig may be something in the tissue or in the texture of the twig itself; but if we carry our inquiry backwards, we shall find that the particular form of the texture results from the particular way in which the molecules are determined during the formation of the texture. Again, on the other hand, that which determines that some particular bud rather than some other should be developed into a branch may, as Mr. Chauncey Wright suggests, be the simple accident which leads to that bud being better supplied with nutriment, light, air, and other favourable conditions. But such an accident can lead to the development of the bud into the branch only through the determination of molecular motion. The selection of that particular bud to be the future branch may be due to these accidental circumstances. But this mode of accidental selection does not explain the special arrangements of the branches. It does not account for the objective idea in that arrangement, unless we suppose that these accidents occur according to a plan and not according to chance. Natural selection never can explain the objective idea in nature unless we suppose the selection to be made according to a design or plan. Mr. Darwin has developed a new and most important idea; but his theory can never, from its very nature, explain the mystery of the organic world. There must be a *determining* cause in the background of all natural selection working out the objective idea. This I trust will be rendered more evident when we come to consider determination of motion in relation to Final Causes.

But there is not merely a unity of plan to be accounted for, but also a unity of purpose. Things in nature are not only related to one another in form, but they stand related as means to ends. And this relationship is as all-pervading as that of form. There

is not an object in nature that does not stand in the relationship of a *means* to something as an *end*. And there exists a unity in the ends as well as in the forms. All molecular motions must consequently have this double relationship of plan and purpose. How, then, is all this order and unity both of plan and purpose in molecular motions to be accounted for?

Molecular Motion in relation to unity of plan.

I shall now consider the explanation of molecular motion in regard to the *Form* of objects. The consideration of molecular motion in relation of Means to Ends must be deferred till a future occasion.

The objects of nature, as we have seen, are built up molecule by molecule, and are thus the products of molecular motion. Energy is that which moves or transports the molecules in the building-up process; but it is not the mere transport of the molecules, as has been repeatedly shown, which gives to the object produced its form. The form assumed is due, not to the motion of the molecules, but to the determination of that motion—to the way in which the motions are guided and adjusted in relation to one another. It is not the energy which conveys the bricks that accounts for the form of the house, but that which guides and directs the energy. So far as the form of the house is concerned, it is a matter of indifference whether the bricks are conveyed on the backs of labourers or transported by a steam-crane. In like manner, in accounting for organic forms, we must exhibit not the mere energy which moves the molecules, but that which directs and guides the energy.

But it has been already proved that energy cannot be determined by energy; consequently that which determines energy is not itself an energy. Therefore the thing which we are in search of, which accounts for the order and arrangement prevailing in the molecular movements in nature, is a something not of the nature of a force or an energy.

The question now to be considered is, Can this marvellous adjustment of molecular motions be explained by any thing which is found within the domains of chemistry and physics? The advocates of the physical theory must afford us some explanation of the cause of the determination of molecular motion derived from physics and chemistry, if their theory in reality rests upon a true foundation.

The chief argument in favour of this theory seems to be the one to which allusion has already been made, viz. that all the energies in nature to which the term "vital" has been applied evidently have a chemical or a physical origin. For example, the vital energies of our bodies are derived from the food we eat, the water we drink, and the air we breathe; they therefore ex-

isted first under the form of chemical affinities. The same is the case in regard to plants; all the energies in operation in the plant are in like manner derived from the nutriment received through its leaves and rootlets.

This, doubtless, is true; but it is of no service to the physical theory, as we have seen; for the fundamental question is not what are the energies in operation, but what is it that determines their mode of operation? It does not necessarily follow, as the advocates of the physical hypothesis would seem to suppose, that because the energies which move the molecules had a chemical or a physical origin, these energies have the same mode of operation as the chemical or the physical energies have. It does not follow that these energies are either chemical or physical merely because they have had a chemical or a physical origin. Animal heat is derived from the chemical energies of the food we eat; the mechanical power by which we raise our arm or move our legs is also derived from the same source; but we do not on this account, as has already been stated, call either animal heat or the power by which we move our limbs chemical energy. We call it physical and mechanical energy. The energy is no longer chemical after it has changed its mode of operation. Then, if there are energies in organic nature which operate in a different way from those which we call chemical and physical, we have no warrant for calling them either chemical or physical merely because they may have had a chemical or a physical origin. But if energies are to be named according to their mode of operation (which is the practice in science), then energies differing from those of chemistry and physics must have a name by which they are to be distinguished. Why not call them "vital energies"?

But the advocates of the physical hypothesis do not admit that there exists in organic nature any form of energy different in character from that to be found in the inorganic world.

We shall now consider whether any thing which that school has advanced on the subject does in any way explain how molecular motions are determined according to the objective idea in nature. Energy, chemical and physical, accounts for molecular motions in organic nature; but how is it to account for the determination of those motions? If the determinations of molecular motion are to be attributed to these energies, it must be to their modes of operation—the *way* in which the energies are exerted—and not to the mere exertion itself. Suppose that the determinations of molecular motion could be accounted for from the known *modes* of the operation of physical energies. The ultimate problem would then be, What is it that determines those modes of operation? In other words, the problem would resolve itself into this, viz. What is the cause of the determination of

physical energies ? What is it that directs the operation of those energies.

But in ascertaining whether chemistry and physics can explain the mystery of nature, the point we have to consider is whether or not there is any thing in the known *modes* of operation of physical and chemical energies which can in any way account for the determination of molecular motion. If the advocates of the physical theory can show that the modes of operation of those energies do explain the determinations of molecular movement, then their theory is established, and the ulterior question as to what is the cause of those modes of operation will be worthy of consideration, for it will then in reality be the grand problem of nature. But if the modes of operation of physical and chemical agencies do not account for the determination of molecular motion, the physical theory must be abandoned, and the solution of the mystery of life and nature must be sought for somewhere else than in chemistry and physics.

Enormous advance has been made in molecular physics and in the science of energy of late years. But it has not thrown much additional light on the cause of the determination of molecular motion, the reason being that the discoveries relate more to the quantitative relationships of energy than to the modes of its operation. It has been found that the total quantity of energy remains constant, that whatever disappears under one form reappears under some other form, and that, whatever may be its form, its amount can be determined in absolute measure. Take for example Heat, the form of energy in regard to which the greatest advance has been made. Heat has been demonstrated to be a mode of motion ; and the amount of energy represented by a given mass of any substance raised by a given number of degrees in temperature can be determined in mechanical units.

We know heat to be some mode of molecular motion ; but we do not as yet know with certainty what that mode is. Most physicists suppose it to consist of a sort of vibratory or oscillatory motion of the molecules, while others conceive it to consist of some sort of molecular vortices. But although the mode of motion has not been determined, nevertheless the velocity of the moving molecules can be estimated. This, however, does not enable us in any way to explain how heat can determine molecular motion in organic nature. If we knew the nature of that mode of motion which constitutes heat, it might possibly be of some service ; but a knowledge of the *quantity* of motion can throw no light on the matter. Heat no doubt is an essential condition to the formation and growth of all living things, whether plants or animals ; but heat is evidently not the determining cause

of the motion of the molecules. We may in thought conceive heat to be a cause of the motion of the molecules; but we cannot conceive any adaptation in the mode of operation of heat that can explain the determination or direction given to the molecules. Whatever may be the nature of that molecular motion called Heat, we have simply a repetition of the same mode of motion; and there is consequently nothing in it to account for the endless diversity of molecular motion which exists in organic nature. Heat, instead of tending to build up complex organic structures, tends, on the contrary, to produce the opposite result. The tendency of heat is to produce homogeneity—to reduce all bodies to one molecular condition. Its direct tendency is not to build up, but to tear asunder and break down. In short, Heat tends to produce *dissolution,* not *evolution.*

If we are in ignorance as to the nature of that mode of motion which constitutes Heat, we are still more so in regard to that mode of motion called Electricity. Physicists have speculated on the nature of Heat; but no physicist of note, so far as I am aware, has even hazarded a conjecture as to what is the nature of that mode of motion called Electricity. Who, then, can assert that there is any thing in the nature of electricity that will account for the determination of molecular motion in organic nature? The mere energy of electricity, like any other form of energy, may be conceived to produce molecular motion; but mere energy will not determine motion. If there be any thing in Electricity that can account for the determination of motion, it is not its energy, but the *mode* of operation of the energy. But as to what this mode actually is we know nothing whatever. We know the effects which this energy produces on masses of matter, but not the nature of the molecular effects produced. Of that particular thing connected with electricity which could be of any possible service to us in reference to the question at issue we at present know absolutely nothing.

But notwithstanding our ignorance in regard to the nature of that mode of motion called Electricity, we are perfectly able to determine from the character of the effects produced by it that, even though our knowledge of it were perfect, it would not afford us any explanation of the cause of the determination of molecular motion in the organic world. We know that electricity, like heat, is a simple repetition of the same mode of motion. The mode of motion in one part of the telegraphic wire is the same in all other parts; and such as it is in one wire, so is it in all other wires. And what holds true of Heat and Electricity holds equally true of Magnetism, Light, and all other forms of physical energy; and it is needless to say that what holds true of physical energy holds equally true of chemical energy.

Molecular physics has made great advance of late years; but it has not made much advance in that particular direction which can be of service in explaining how molecular motion in organic nature is determined. It is thought, however, by the advocates of the physical school that although at present we are unable to explain how organic nature can be built up by the play of the ordinary chemical and physical forces, yet at some future day, when we shall have come to know far more of molecular physics than we do at present, then we may be able to explain the mystery. This is the cherished hope of modern Evolutionists, and of the advocates of the physical theory of life. But it is a mental delusion, a dream which will never be realized. A little consideration might satisfy any one that Chemistry and Physics will never explain the mystery of nature.

The terms Light, Heat, Electricity, Magnetism, &c. are different names which we apply to different modes of molecular motion; and it is true that at present little is known regarding the nature of these modes of motion; but notwithstanding this we have reason to conclude that, although we knew all that absolutely can be known regarding them, yet it *would not afford us any explanation* of the cause of the determination of molecular motion in organic nature.

The character of a cause may often to some extent be judged indirectly from the nature of the effects produced. It is from the effects produced that we know, for example, that that mode of molecular motion called Heat differs from that mode called Electricity. The effects do not as yet enable us to determine wherein this difference consists; but it enables us to conclude with certainty that there is a difference. Effects which are electrical we refer to that unknown mode of motion called Electricity. We do not refer them to that mode called Heat, because the effects are different from those which we ascribe to Heat. Each mode of motion, each energy is distinguished by the effects which it produces. Determination of the molecules of matter according to the objective idea of a plant or an animal, is an effect which is constantly taking place in organic nature. To attribute this effect to Electricity, for example, would be far more absurd than to attribute electrical effects to gravitation or to heat; for the difference between this effect and any electrical effect is immeasurably greater than between electrical effect and any effects produced by heat or by gravitation or any other of the forces of inorganic nature. It would be far more rational to attribute all the phenomena of the inorganic world, say, to heat, than to attribute the determination of molecular motion in the organic world to chemical and physical energies.

It must now be obvious that nothing which can be deter-

mined by the comparative anatomist, no biological researches, no microscopic investigations, no considerations regarding natural selection or the survival of the fittest can solve the great problem of nature; for it lies in the background of all such investigations. The problem is molecular. From the hugest plant and animal on the globe down to the smallest organic speck visible under the microscope, all have been built up molecule by molecule; and the problem is, to explain this molecular process. If one plant or animal differs from another, or the parent from the child, it is because in the building-up process the determinations of molecular motion were different in the two cases; and the true and fundamental ground of the difference must be sought for in the cause of the determination of molecular motion. Here in this region the doctrine of natural selection and the struggle for existence can afford no more light on the matter than the fortuitous concourse of atoms and the atomical philosophy of the ancients. This, I trust, will be rendered still more evident when we come to examine in detail the arguments advanced by modern evolutionists in support of their fundamental hypothesis, "that the whole world, living and not living, is the result of the mutual interaction, according to definite laws, of the forces possessed by the molecules of which the primitive nebulosity of the universe was composed."

II. *On a new Lantern-Galvanometer.* *By* ALFRED M. MAYER, *Ph.D., Professor of Physics in the Stevens Institute of Technology, Hoboken, N. J., U.S. America*[*].

ON the 21st of December, 1871, I delivered a lecture on Magnetism before the American Institute at the Academy of Music in the city of New York. It was necessary for the experimental discussion I then made of the earth's magnetism to use a galvanometer so constructed that the deflections of its needle would be visible to a large audience; at the same time the astatic condition of this needle had to be so controlled that it could readily be altered during the progress of the lecture; while, finally, the arrangement of the damping-magnets had to be such as allowed me instantly to bring the needle into the magnetic meridian when disturbed therefrom whenever I set in action the huge electromagnet used on that occasion. Indeed one of the principal uses to which this galvanometer was applied in the lecture was the exploration of the magnetic condition of the space surrounding this electromagnet. This I accomplished

by rotating around the line of " the dip," as an axis, wire coils at various distances and positions, and leading the induced magneto-electric currents through the galvanometer.

The lantern-galvanometer, which I will now proceed to describe, I devised on the 13th of last November; and as subsequent work with it has convinced me of its value in the lecture-room, I have decided to give it this formal publication.

Referring to the figure, M is a plane mirror inclined 45° to the vertical. In front of this are the back condensing-lenses of an oxyhydrogen lantern; while the front lens of the condenser is placed in a horizontal position at *c, above* the mirror. The back condensing-lenses are of such curvatures that when the calcium light is placed about two inches from the one nearest it, a nearly parallel beam issues from them to fall upon M, thence to be reflected to the upper condensing-lens at *c**, on which rests a disk of glass on whose border is photographed a divided circle. In the centre of this disk is a short needle-point on which freely rotates a magnetic needle. Above the needle is the projecting lens L, the pencils from which are reflected in any desired direction by means of the plane mirror R, which revolves on a horizontal axis, and has also a motion in azimuth round the axis of the lens†.

The horizontal condensing-lens is 5 inches in diameter; and the magnetic needle is 4 inches long. With this arrangement I have obtained sharp and bright images of the graduated circle 16 feet in diameter.

To deflect this needle by means of an electric current, I place as close to the condensing-lens as possible the two vertical wire spirals S, S, formed of $\frac{1}{10}$-inch copper wire of *square* section so as to bring the convolutions as close together as possible. The turns of the spirals are separated with very thin vulcanite ribbon

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* This arrangement of lenses, which is due to President Morton, gives a bright and uniformly illuminated field free from coloration.

In the Quarterly Journal of Science, October 1871, is a report of Professor Morton's account of this invention ["the vertical lantern"], delivered before the American Institute, as follows :—"The original idea and general plan of the instrument shown was, as the speaker stated, due to Professor J. P. Cooke, of Cambridge—his own work in connexion with it being confined to the devising of a convenient mechanical arrangement of parts, the improvement of the combination of condensing-lenses with the reflecting lenses [mirror?] so as to secure a white and evenly illuminated field on the screen, and the discovery that an ordinary silvered mirror would serve for the final reflection as efficiently as a metal speculum or glass silvered by Foucault's plan, which are so difficult to obtain and keep in order."

† In a college course of lectures it is sometimes convenient to reflect the image of circle and needle down on a white-covered table below the class. The galvanometer can then be placed on the lecture-table.

coated with paraffine, and are wrapped on the faces of vulcanite disks. The spirals have an internal diameter of 4 inches and an external diameter of 10 inches; and each contains 49 feet of wire in 26 turns. The four terminals of the spirals are connecting-screws, two of which serve to join the spirals so that a current will circulate in the same direction in both. The spirals are so placed that a line joining their centres will pass through the centre of the magnetic needle.

The vertical lantern rests on a base $3\frac{1}{2}$ feet long, with guides on its sides, between which slide boards carrying two bar-magnets, A and B, 15 inches long and 1 inch in diameter, as shown in the figure. These magnets can not only approach to and re-

cede from the lantern, and thus alter their distances from the galvanometer-needle, but they can also rotate around their centres on vertical axes. The like poles of the magnets and of the needle point in the same direction; and by sliding the magnets to or from the lantern-needle we render the latter more or less astatic. Also, in case the needle should not hold to the meridian as you approach the magnets, it can be made to do so by rotating one or both of them in the horizontal plane; and thus also can be neu-

tralized any exterior disturbance which may tend to deflect the needle from the magnetic meridian.
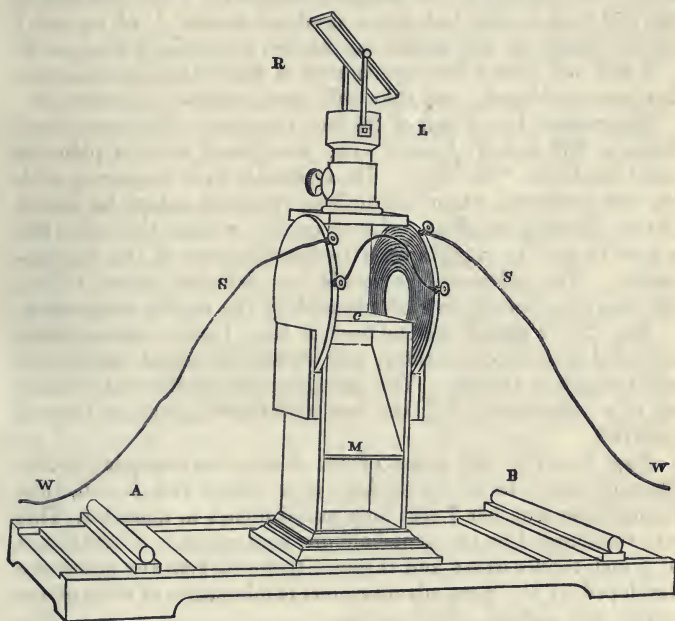
The needle may be also rendered astatic in the usual way by suspending it by a silk fibre, and attaching to this needle a wire which passes through a hole in the condenser and in the inclined mirror and carries beneath the latter another needle with poles reversed.

In working with thermal currents we use a smaller needle and condenser, which allows the spirals to approach nearer; but for thermal currents it is better to wind close round the needle a flat coil only one wire in breadth, and to use a suspended astatic system, of which the *lower* needle is the stronger and is under the control of the damping-magnets*. The breadth of the coil used in this last device need not exceed $\frac{1}{20}$ of an inch; and its image on the screen can answer for a rough zero-point.

I will now give a few experiments in which this galvanometer has been employed; and they will serve to show its usefulness.

Experiment 1.—A coil of $2\frac{1}{2}$ feet diameter, containing forty turns of 300 feet of $\frac{1}{10}$-inch wire, was placed with its plane at right angles to "the dip." Its terminals were connected with the galvanometer, whose needle was rendered astatic by means of the damping-magnets. I now quickly rotated the coil 180° round an axis at right angles to the direction of the dipping-needle. The galvanometer-needle was deflected about 12° by the magneto-electric current induced by the earth's magnetism.

Exp. 2.—I placed the coil used in Exp. 1 on a wooden wheel provided with a commutator, and rotated it round an axis at right angles to the dip. The galvanometer-needle went steadily up to a deflection of 85°, and was held there as long as the coil revolved.

Exp. 3.—The two cores of the large electromagnet of the Stevens Institute of Technology were placed end to end, thus forming one iron bar 7 feet long and 6 inches in diameter. This was surrounded by its eight bobbins, containing in all 2000 feet of $\frac{1}{5}$-inch copper wire; and through them was sent the electricity developed by the most advantageous combination of sixty plates of zinc and carbon, 10×8 inches.

A coil of 20 inches diameter, formed of one turn of $\frac{1}{20}$-inch wire, was rotated 180° round a vertical axis $3\frac{1}{2}$ feet from the end of the magnet. The needle was deflected 3°.

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* The upper needle of this astatic combination swings in the interior of the coil which encloses both the needle and the condenser c; the lower needle swings under the inclined mirror M, and is attached to the upper needle by means of a stiff wire, which passes through holes in the condenser and in the inclined mirror. In another combination I have placed this lower needle *above* the coil, and have "damped" it by means of a magnet placed above the reflector R.

Exp. 4.—A coil of 20 inches diameter, having five turns of $\frac{1}{20}$-inch wire, was rotated 180° round a vertical axis at a distance of $3\frac{1}{2}$ feet from end of magnet. Deflection of needle was 30°.

Exp. 5.—Same as Exp. 4, only coil had ten turns of wire instead of five. Galvanometer-needle deflected 50° to 60°.

Exp. 6.—A coil of 20 inches diameter, formed of ten turns of $\frac{1}{20}$-inch wire, was revolved 180° round a vertical axis $6\frac{1}{2}$ feet from end of magnet. Deflection of needle 22°.

Exp. 7.—The coil used in Exp. 6 was placed 3 feet 8 inches above centre of axis of the magnet, and revolved 180° round a vertical axis; the needle was deflected 80°.

Exp. 8.—A coil of $2\frac{1}{2}$ feet diameter, formed of forty turns of 300 feet of $\frac{1}{10}$-inch wire, was placed 28 feet distant from the centre of the magnet, and with its plane coinciding with the plane of the magnet's equator. On rotating it round a vertical axis the needle was deflected 20°.

The following experiments will show the excellent proportions (arrived at by a long series of experiments) of the coil used in Exps. 1, 2, and 8, for the evolution and study of the electric currents induced by the earth's magnetism.

Exp. 9.—The coil used in Exps. 1, 2, and 8 was laid on a table, and its terminals connected with a galvanometer which is used in connexion with Nobili's thermo-electric pile. The needles of this instrument made one oscillation in nine seconds. I lifted the east side of the coil only six inches; the needle was deflected 10°. Lifting the same side nine inches, the needles went to 22°. I now placed the coil in a north and south vertical plane; and suddenly tilting its top six inches to the east or to the west, the needles went to 60°. Tilting the coil nine inches sent the needles with a blow against the stop at 90°.

The advantages of the new galvanometer may be summed up in a few words. It gives on the screen a bright clear image of only the graduated circle and of the needle. It can readily be rendered more or less astatic to adapt it to the character of the electric currents worked with. The direction of its needle is completely under the control of the damping-magnets; and, finally, it is of simple construction, and can be rapidly adjusted to the requirements of any special experiment.

III. *On the steady Flow of a Liquid. By the late* HENRY
MOSELEY, *M.A., D.C.L., Canon of Bristol, F.R.S., Corre-
sponding Member of the Institute of France, &c. Edited by*
WALTER R. BROWNE, *M.A., Fellow of Trinity College, Cam-
bridge.*

[With a Plate.]

Prefatory Note.

THE following paper completes the series on the steady
    Flow of Liquids published in the Philosophical Magazine
by the late Canon Moseley. He did not live to revise it for
publication; that task was, by his request, entrusted to me.
The paper, however, was practically complete; and all that I
have found necessary, beyond numbering the equations and su-
perintending the publication, has been to make a very few ne-
cessary additions and corrections, the more important of which
are noticed where they occur.

The circumstances, however, under which the paper was
finished seem to me to demand, and will, I think, excuse some-
thing beyond this brief explanation. It was the work on which
Canon Moseley was engaged when seized with his last illness;
and during that illness it still occupied his thoughts. Some
time after all hope of recovery had ceased, and when death had
already come very near, there was a short rally of strength; and
he then dictated to his daughter the last three paragraphs. No
one can read these without being struck by their composure,
their courtesy, and perfect clearness of thought; and no one
would, I believe, suspect under what circumstances they were
written down. Nevertheless that the mind, even in full view of
death, should still move freely along the paths to which it is
accustomed may be, although a striking, not a very rare pheno-
menon. But that which as men of science we may well note is
this:—that whereas it is often asserted and oftener assumed that
a deep study of the laws of nature forbids the mind to acknow-
ledge any thing beyond those laws, we here see one who in the
very last hours of life could still pursue that difficult branch of
earthly knowledge in which his high distinction had been won,
and who could also turn directly from such pursuit to receive
the ministrations of that religion and that church in which he
had lived, and in which he was well content to die.

*General Conditions of the Uniform Flow of a Liquid in a closed
    Channel of any given geometrical form which it enters from
    a reservoir.*

By the uniform flow of a liquid is meant here (as before in

Fig. 3.
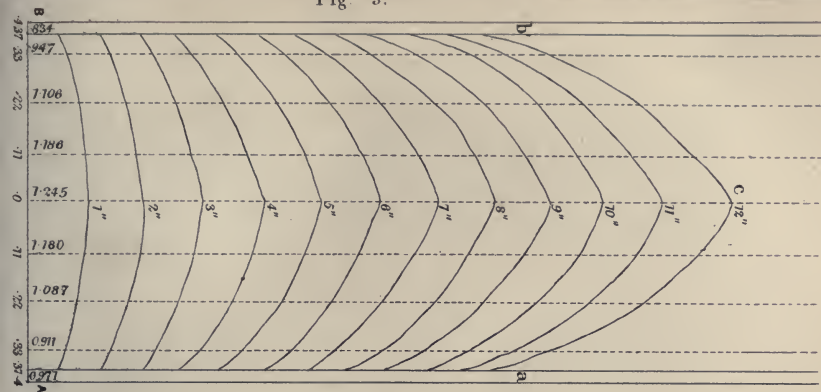
Fig. 4.
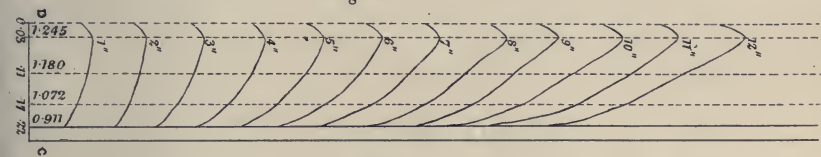
Fig. 1.
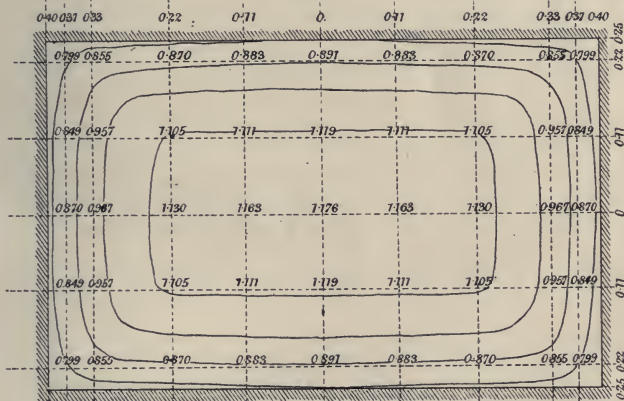
Fig. 2

these papers) that state of steady motion to which it would attain in a channel of uniform section whose internal surface was everywhere of the same degree of roughness, and whose direction was straight and its shape constant. The liquid would under these circumstances flow in filaments parallel to one another and to the sides of the channel, having different velocities.

By equation (2)*,

$$U = U_1 + U_2 + U_3 + U_4,$$

where

$U =$ *work* done per unit of time on the liquid which enters the pipe by the pressure of that in the reservoir.

$U_1 =$ *work* carried away per unit of time by the liquid which flows from the extremity of the pipe.

$U_2 =$ *work* expended on the various resistances which are opposed to the descent of the liquid in the reservoir and to its passage from the reservoir through its aperture into the pipe.

$U_3 =$ *work* expended on the resistance of the internal surface of the pipe to the flow of the liquid along it.

$U_4 =$ *internal work* of the resistance of the films to the flowing of each film over the surface of the next in succession.

Let a plane be imagined to intersect the liquid in a direction at right angles to that in which it flows, and let the point in the plane where the filament of maximum velocity intersects it be taken as the origin of the coordinates; also let x and y be the rectangular coordinates of the point in this plane where any other filament intersects it, x being horizontal. Let v be the velocity of this filament. Its displacement in the unit of time over the filament immediately above it will then be represented by $-\left(\dfrac{dv}{dy}\right)dy$, the negative sign being taken because v diminishes as y increases.

But the resistance opposed to the shearing by the horizontal face of the filament is $\mu l \Delta x$; μ representing the unit of resistance, and l the length of the filament. Therefore the *work* done in the unit of time by the resistance of the lower of these two filaments to the motion of the upper is represented by

$$-\mu l \Delta x \left(\frac{dv}{dy}\right) dy.$$

In the same manner the work done by the resistance of the side surface of the filament to the motion of the next contiguous filament sidewise is represented by

$$-\mu l \Delta y \left(\frac{dv}{dx}\right) dx.$$

* Phil. Mag. September 1871.

The whole work of the resistance of this filament is represented therefore by

$$-\mu l \Delta x \left(\frac{dv}{dy}\right) dy - \mu l \Delta y \left(\frac{dv}{dx}\right) dx,$$

$\left(\frac{dv}{dx}\right)$ and $\left(\frac{dv}{dy}\right)$ being the partial differential coefficients of v with respect to x and y; or, passing to the limit, the work of the resistance of this filament is represented by

$$-\mu l \left\{\left(\frac{dv}{dy}\right) + \left(\frac{dv}{dx}\right)\right\} dx\, dy.$$

Therefore the aggregate work U_4 of the resistances of all the filaments is represented by

$$U_4 = -\iint \mu l \left\{\left(\frac{dv}{dy}\right) + \left(\frac{dv}{dx}\right)\right\} dx\, dy.$$

If h represent the actual head of liquid in the reservoir, then, supposing the channel to be horizontal,

$$U = wh \iint v dx\, dy.$$

The effect of the resistance whose work is U_2, however, is practically to diminish the head of liquid h.

Let $\dfrac{h}{\gamma}$ represent, as before, this diminished head; then, if we substitute $\dfrac{h}{\gamma}$ for h in the above equation, we may neglect U_2 in the preceding equation. If moreover we suppose the liquid after leaving the reservoir to descend through an inclined channel whose length is l, we must substitute

$$\left(\frac{h}{\gamma} + l \sin i\right) \text{ for } \frac{h}{\gamma};$$
$$\therefore U = w\left(\frac{h}{\gamma} + l \sin i\right)\iint v dx\, dy.$$

$U_1 =$ half the *vis viva* of the discharge per unit of time

$$= \iint \frac{1}{2}\left(\frac{wv dx\, dy}{g}\right)v^2 = \frac{w}{2g}\iint v^3\, dx\, dy.$$

Hence, neglecting U_2, we have by equation (2),

$$w\left(\frac{h}{\gamma} + l \sin i\right)\iint v dx\, dy = \frac{w}{2g}\iint v^3 dx\, dy + U_3$$
$$- \iint \mu l \left\{\left(\frac{dv}{dy}\right) + \left(\frac{dv}{dx}\right)\right\} dx\, dy. \quad . \ (46)$$

Differentiating twice and dividing by $dx\,dy$,

$$w\left(\frac{h}{\gamma}+l\sin i\right)v=\frac{w}{2g}v^3+\frac{d\mathrm{U}_3}{dx\,dy}-\mu l\left\{\left(\frac{dv}{dy}\right)+\left(\frac{dv}{dx}\right)\right\}.$$

Observing that $\dfrac{d\mathrm{U}_3}{dx\,dy}=0$, and transposing,

$$\left(\frac{dv}{dy}\right)+\left(\frac{dv}{dx}\right)=\frac{wv^3}{2\mu gl}-\frac{w}{\mu l}\left(\frac{h}{\gamma}+l\sin i\right)v.$$

Let

$$\frac{w}{2\mu gl}=\alpha^2,\quad \frac{w}{\mu l}\left(\frac{h}{\gamma}+l\sin i\right)=\beta^2;$$

$$\therefore \left(\frac{dv}{dy}\right)+\left(\frac{dv}{dx}\right)=\alpha^2 v^3-\beta^2 v,$$

and

$$\left(\frac{dv}{dy}\right)dx+\left(\frac{dv}{dx}\right)dx=(\alpha^2 v^2-\beta^2)v\,dx;$$

but*

$$dv=\left(\frac{dv}{dy}\right)dy+\left(\frac{dv}{dx}\right)dx;$$

$\therefore$ adding,

$$dv+\left(\frac{dv}{dy}\right)dx=(\alpha^2 v^2-\beta^2)v\,dx+\left(\frac{dv}{dy}\right)dy;$$

transposing,

$$dv-(\alpha^2 v^2-\beta^2)v\,dx=\left(\frac{dv}{dy}\right)(dy-dx);$$

$$\therefore \frac{dv}{(\alpha^2 v^2-\beta^2)v}-dx=\frac{\left(\dfrac{dv}{dy}\right)(dy-dx)}{(\alpha^2 v^2-\beta^2)v}.$$

Since the left-hand member of this equation is an *exact differential*, the right-hand must be one also†. This condition is satisfied if $\dfrac{\left(\dfrac{dv}{dy}\right)}{(\alpha^2 v^2-\beta^2)v}$ is a function of $(y-x)$; but it may be any function.

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* In the original MS. this expression has been inadvertently written $\left(\frac{dv}{dy}\right)dx+\left(\frac{dv}{dx}\right)dy$. I have corrected this error as far as it goes; it does not affect the form of equation (48).—W. R. BROWNE.

† This is the well-known method of Lagrange.

Let

$$\frac{\left(\dfrac{dv}{dy}\right)}{(\alpha^2 v^2 - \beta^2) v} = \phi'(y - x),$$

where

$$\phi'(y - x) = \frac{d\phi(y - x)}{dy - dx},$$

ϕ being any function of $(y - x)$;

$$\therefore \frac{dv}{(\alpha^2 v^2 - \beta^2) v} - dx = \phi'(y - x)(dy - dx);$$

$$\therefore \int_{v_0}^{v} \frac{dv}{(\alpha^2 v^2 - \beta^2) v} - x = \phi(y - x) - \phi(0). \quad . \quad . \quad (47)$$

But

$$\frac{1}{(\alpha^2 v^2 - \beta^2) v} = -\frac{1}{\beta^2 v} + \frac{\alpha}{2\beta^2} \frac{1}{(\alpha v - \beta)} + \frac{\alpha}{2\beta^2} \frac{1}{(\alpha v + \beta)};$$

$$\therefore \int_{v_0}^{v} \frac{dv}{(\alpha^2 v^2 - \beta^2) v} = \frac{1}{\beta^2} \left(\log_\epsilon \frac{v_0}{v} + \frac{1}{2} \log_\epsilon \frac{\alpha v - \beta}{\alpha v_0 - \beta} + \frac{1}{2} \log_\epsilon \frac{\alpha v + \beta}{\alpha v_0 + \beta} \right)$$

$$= \frac{1}{2\beta^2} \log_\epsilon \left\{ \left(\frac{v_0}{v} \right)^2 \left(\frac{\alpha^2 v^2 - \beta^2}{\alpha^2 v_0^2 - \beta^2} \right) \right\}$$

$$= \frac{1}{2\beta^2} \log_\epsilon \left\{ \frac{1 - \left(\dfrac{\beta}{\alpha v} \right)^2}{1 - \left(\dfrac{\beta}{\alpha v_0} \right)^2} \right\};$$

$$\therefore \frac{1}{2\beta^2} \log_\epsilon \left\{ \frac{1 - \left(\dfrac{\beta}{\alpha v} \right)^2}{1 - \left(\dfrac{\beta}{\alpha v_0} \right)^2} \right\} - x = \phi(y - x) - \phi(0);$$

$$\therefore \frac{1 - \left(\dfrac{\beta}{\alpha v} \right)^2}{1 - \left(\dfrac{\beta}{\alpha v_0} \right)^2} = \epsilon^{2\beta^2 \left\{ x + \phi(y - x) - \phi(0) \right\}};$$

$$\therefore 1 - \left(\frac{\beta}{\alpha v} \right)^2 = \left\{ 1 - \left(\frac{\beta}{\alpha v_0} \right)^2 \right\} \epsilon^{2\beta^2 \left\{ x + \phi(y - x) - \phi(0) \right\}};$$

$$\therefore v^2 = \frac{\left(\dfrac{\beta}{\alpha} \right)^2}{1 - \left\{ 1 - \left(\dfrac{\beta}{\alpha v_0} \right)^2 \right\} \epsilon^{2\beta^2 \left\{ x + \phi(y - x) - \phi(0) \right\}}}.$$

Substituting for α and β their values,

$$v^2* = \frac{2g\left(\dfrac{h}{\gamma} + l\sin i\right)}{1-\left\{1-2g\left(\dfrac{h}{\gamma}+l\sin i\right)\dfrac{1}{v_0^2}\right\}\epsilon^{\frac{2w}{\mu}\left(\frac{h}{\gamma l}+\sin i\right)\left\{x+\phi(y-x)-\phi(0)\right\}}} . \quad (48)$$

The conditions of equation (46) are satisfied by the above equation, whatever may be the form or value of the function $\phi(y-x)$. But these conditions are not the only ones to which the flow of the liquid is subjected. They do not include the form of the channel, or the degree of roughness or smoothness of its sides. The term U_3 in equation (46) represented these; but it disappeared in the double differential, and has no place in equation (47), from which equation (48) is deduced. The indeterminateness of $\phi(y-x)$ results from the neglect of this condition; and the function is to be determined by taking it into account. Let us suppose it to be so determined.

Liquid Films.

If in any cross section a curve be taken whose equation is

$$x+\phi(y-x)-\phi(0)=\rho,$$

where ρ is constant, that curve will represent the intersection of it by *a film.* For, by equation (48), the velocity v of the liquid at every point in that curve will be the same. By varying the values of ρ, all the films of a given stream flowing uniformly may thus be determined. In closed channels of symmetrical forms and uniform roughness the velocities of such particles as flow nearly in contact with the sides approach probably to equality. A film nearly in contact with the sides has therefore nearly the form of the channel itself; and as the films are geometrically similar, it follows that approximately all the films, from the filament of maximum velocity to the internal surface of the channel, take approximately the geometrical form of that surface. The degree of that approximation can only be determined by comparing theoretical results founded upon it with the results of experiment. *That* is the object of what remains of this paper.

* To compare this equation with equation (10) (Phil. Mag. September 1871), we must make $\sin i=0$, $\gamma=1$, $2gh=v^2$, $\dfrac{h}{l}=i$. We shall thus obtain

$$v^2 = \frac{v^2}{1-\left(1-\dfrac{v^2}{v_0^2}\right)\epsilon^{\frac{2wi}{\mu}\left\{x+\phi(y-x)-\phi(0)\right\}}}.$$

Assuming $x+\phi(y-x)-\phi(0)=r$, this equation becomes identical with equation (10).

The Velocity (at any point) of the Flow of a Liquid through a straight pipe of any shape.

Let it be supposed that the liquid fills the pipe, and that it arranges itself in *films* geometrically similar to the internal surface of the pipe, the molecules in each film moving with the same velocity, but those in different films with different velocities. The sections of these films made by a plane perpendicular to the axis being geometrically similar, let straight lines be supposed to be similarly placed in them, one in each section, and let the length of any one of these lines be represented by r. Let ψ represent the area of that section for which $r = 1$, and $2\psi_1$ the perimeter of that section. The area of any other section will then be represented by $r^2\psi$, and its perimeter by $2r\psi_1$.

Adopting the same notation and reasoning in the same way as before (p. 186, Phil. Mag. Sept. 1871),

$$U = h\int_0^r wv(2\psi rdr), \quad U_1 = \int_0^r \frac{w(2\psi rdr)v}{2g}v^2,$$

$$U_4 = -\int_0^r 2\psi_1 rl\mu\left(\frac{dv}{dr}\right)dr;$$

$$\therefore 2\psi wh\int_0^r vrdr = \frac{\psi w}{g}\int_0^r v^3rdr + U_2 - 2\psi_1 l\int_0^r \mu\left(\frac{dv}{dr}\right)rdr. \quad . \quad (49)$$

Differentiating and considering U_2 constant,

$$2\psi whvr = \frac{\psi w}{g}v^3r - 2\psi_1 l\mu\left(\frac{dv}{dr}\right)r.$$

Taking $2gh = v^2$, and $\dfrac{\psi}{\psi_1} = \Psi$, and reducing,

$$\therefore \Psi(v^2 - v^2)v = -\frac{2gl\mu}{w}\left(\frac{dv}{dr}\right);$$

$$\therefore \left(\frac{dv}{dr}\right)\frac{1}{(v^2 - v^2)v} = -\frac{\Psi w}{2gl\mu};$$

$$\therefore \left(\frac{dv}{dr}\right)\left\{\frac{1}{v} + \frac{1}{2}\left(\frac{1}{v-v} - \frac{1}{v+v}\right)\right\} = -\frac{\Psi wv^2}{2gl\mu}$$

$$= -\frac{\Psi wh}{l\mu} = -\frac{\Psi wi}{\mu}.$$

Integrating between the limits 0 and r,

$$\log_e\left\{\left(\frac{v}{v_0}\right)\left(\frac{v^2 - v_0^2}{v^2 - v^2}\right)^{\frac{1}{2}}\right\} = -\frac{\Psi wir}{\mu};$$

$$\therefore \left(\frac{v}{v_0}\right)\left(\frac{v^2-v_0^2}{v^2-v^2}\right)^{\frac{1}{2}} = \epsilon^{-\frac{\Psi wir}{\mu}}; \quad \ldots \ldots \quad (50)$$

$$\therefore v^2 = \frac{v^2}{1+\left\{\left(\frac{v}{v_0}\right)^2-1\right\}\epsilon^{\frac{2\Psi wir}{\mu}}}. \quad \ldots \quad (51)$$

Taking, as before, $\dfrac{wi}{\mu}=\gamma$, and neglecting, as before, the work accumulated in the liquid which escapes per unit of time (Phil. Mag. Sept. 1871, p. 193),

$$v = v_0\epsilon^{-\Psi\gamma r}. \quad \ldots \ldots \quad (52)$$

The discharge from a closed straight channel of any given geometric form in terms of the maximum velocity.

Assuming R to be the value of r for that film which is in contact with the interior of the channel, and Q to represent the discharge per unit of time, and reasoning as before (Phil. Mag. Sept. 1871, p. 195),

$$Q_R = \int_0^R (2\psi r\,dr)v = 2\psi\int_0^R vr\,dr = 2\psi v_0\int_0^R \epsilon^{-\Psi\gamma r}r\,dr \text{ by equation (52)},$$

$$\int \epsilon^{-\Psi\gamma r}r\,dr = -\frac{1}{\Psi\gamma}\epsilon^{-\Psi\gamma r}r + \frac{1}{\Psi\gamma}\int_0^R \epsilon^{-\Psi\gamma r}dr = -\frac{1}{\Psi\gamma}\epsilon^{-\Psi\gamma r}r$$
$$-\frac{1}{(\Psi\gamma)^2}\epsilon^{-\Psi\gamma r} + C,$$

$$\int_0^R \epsilon^{-\Psi\gamma r}r\,dr = -\frac{1}{\Psi\gamma}\epsilon^{-\Psi\gamma R}R - \frac{1}{(\Psi\gamma)^2}(\epsilon^{-\Psi\gamma R}-1)$$
$$= \frac{1}{(\Psi\gamma)^2}\left\{1-(\Psi\gamma R+1)\epsilon^{-\Psi\gamma R}\right\},$$

$$\therefore Q_R = \frac{2\psi}{(\Psi\gamma)^2}\left\{1-(\Psi R\gamma+1)\epsilon^{-\Psi R\gamma}\right\}v_0, \quad \ldots \quad (53)$$

in which expression γ is dependent on the work U_2 lost in the descent of the water in the reservoir and in its passage into the pipe, and on the amount of work U_1 which it carries away with it on leaving the pipe, but is approximately constant for the same pipe and the same reservoir under different heads of water.

Equation (53) may be put under another form.

Let Ω = actual section of the pipe, χ = actual perimeter. Then

$$\Omega = R^2\psi, \quad \chi = 2R\psi_1,$$
$$\psi = \frac{\Omega}{R^2}, \quad \frac{\Omega}{\chi} = \frac{R\psi}{2\Psi_1} = \frac{1}{2}R\Psi;$$

$$\therefore \ \Psi R = \frac{2\Omega}{\chi}, \quad \Psi = \frac{2\Omega}{R\chi};$$

$$\therefore \frac{2\psi}{\Psi^2} = \frac{2\dfrac{\Omega}{R^2}}{\dfrac{4\Omega^2}{R^2\chi^2}} = \frac{\chi^2}{2\Omega}.$$

Substituting in equation (53),

$$Q = \frac{\chi^2}{2\Omega\gamma^2}\left\{ 1 - \left(\frac{2\Omega\gamma}{\chi} + 1\right)\epsilon^{-\frac{2\Omega\gamma}{\chi}} \right\}v_0. \quad . \quad (54)$$

This formula has this advantage in its application to Messrs. Darcy and Bazin's experiments, that the numerical values of χ and Ω have in every case been calculated by them.

The discharge Q in terms of the head of water h, and the length of the pipe l.

It may be shown, as in equation (4), that

$$U_1 = \frac{w\psi}{g}\int_0^R v^3 r\,dr = \frac{w\psi v_0}{g}\int_0^R \epsilon^{-3\Psi\gamma r} r\,dr,$$

since $v = v_0 \epsilon^{-\Psi\gamma r}$, or, as in equation (25),

$$U_1 = \frac{w\psi V^3}{9g(\Psi\gamma)^2}\left\{ \epsilon^{3\Psi\gamma R} - 3\Psi\gamma R - 1 \right\};$$

also

$$U_3 = {}^*2R\psi_1 l(\mu_1 + \lambda_1 V^2)V,$$

or, neglecting μ_1 as small compared with $\lambda_1 V^2$,

$$U_3 = 2R\psi_1 l \lambda_1 V^3;$$

whence we obtain, as in equation (26),

$$\frac{w\psi V^3}{9g(\gamma\Psi)^2}\left\{ \epsilon^{3\gamma\Psi R} - 3\gamma\Psi R - 1 \right\} + 4\psi_1\lambda_1 lRV^3 = w\psi\sqrt{2g}h^{\frac{3}{2}}R^2;$$

$$\therefore V^3 = \frac{w\Psi(2g)^{\frac{1}{2}}h^{\frac{3}{2}}R^2}{\dfrac{w\Psi}{9g(\gamma\Psi)^2}\left\{ \epsilon^{3\gamma\Psi R} - 3\gamma\Psi R - 1 \right\} + 4\lambda_1 lR},$$

$$V = \frac{(w\Psi)^{\frac{1}{3}}(2g)^{\frac{1}{6}}h^{\frac{1}{2}}R^{\frac{2}{3}}}{\left\{ \dfrac{w}{9g\gamma^2\Psi}(\epsilon^{3\gamma\Psi R} - 3\gamma\Psi R - 1) + 4\lambda_1 lR \right\}^{\frac{1}{3}}}. \quad . \quad (54A)$$

But $V = v_0 \epsilon^{-\gamma\Psi R}$,

$$\therefore v_0 = V\epsilon^{\gamma\Psi R}; \quad . \quad . \quad . \quad . \quad . \quad . \quad (54B)$$

* *Editor's Note.*—The form of the expression $\mu_1 + \lambda_1 V^2$ is discussed in a note at the end of this paper.

∴ by equation (53),

$$Q = \frac{2\psi V}{(\gamma\Psi)^2}\{\epsilon^{\gamma\Psi R} - \gamma\Psi R - 1\};$$

$$\therefore Q = \frac{2\psi}{(\gamma\Psi)^2} \frac{(w\Psi)^{\frac{1}{3}}(2g)^{\frac{1}{6}}h^{\frac{1}{2}}R^{\frac{2}{3}}(\epsilon^{\gamma\Psi R} - \gamma\Psi R - 1)}{\left\{\dfrac{w}{9g\Psi\gamma^2}(\epsilon^{3\gamma\Psi R} - 3\gamma\Psi R - 1) + 4\lambda_1 lR\right\}^{\frac{1}{3}}};$$

$$\therefore Q* = \frac{(2g)^{\frac{1}{2}}\psi}{(\gamma\Psi)^{\frac{4}{3}}} \cdot \frac{(\epsilon^{\gamma\Psi R} - \gamma\Psi R - 1)h^{\frac{1}{2}}R^{\frac{5}{3}}}{\left\{\dfrac{\epsilon^{3\gamma\Psi R} - 3\gamma\Psi R - 1}{36R} + \dfrac{g\gamma^2\lambda_1 l\Psi}{w}\right\}^{\frac{1}{3}}}. \quad \cdot \quad (55)$$

Neglecting the first term in the denominator and reducing,

$$Q = \left(\frac{\psi}{\Psi^{\frac{5}{3}}}\right) \cdot \frac{2^{\frac{3}{2}}g^{\frac{1}{6}}w^{\frac{1}{3}}(\epsilon^{\gamma\Psi R} - \gamma\Psi R - 1)h^{\frac{1}{2}}R^{\frac{5}{3}}}{\gamma^2\lambda_1^{\frac{1}{3}}l^{\frac{1}{3}}},$$

$$Q = 20{\cdot}691 \times \left(\frac{\psi}{\Psi^{\frac{5}{3}}}\right)\left(\frac{\epsilon^{\gamma\Psi R} - \gamma\Psi R - 1}{\gamma^2\lambda_1^{\frac{1}{3}}}\right)(\sin i)^{\frac{1}{2}}l^{\frac{1}{6}}R^{\frac{5}{3}}. \quad \cdot \quad (57)$$

The discharge of a rectangular pipe.

Let b and c be respectively the breadth and depth of a rectangular pipe internally, and let the line similarly placed in each of the films which is represented by r be its breadth b. Then $R = b$; also the depth of that film whose breadth $(r) = 1$ is $\dfrac{c}{b}$;

$$\therefore \psi = 1 \times \frac{c}{b}, \quad 2\psi_1 = 2 + 2\frac{c}{b}; \quad \therefore \psi_1 = 1 + \frac{c}{b};$$

$$\Psi = \frac{\psi}{\psi_1} = \frac{\dfrac{c}{b}}{1 + \dfrac{c}{b}} = \frac{c}{b+c}. \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (58)$$

If the line (r) similarly placed in each of the films had been its depth c instead of its breadth b, then

$$\Psi = \frac{b}{c+b}. \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (59)$$

* If R be exceedingly small,

$$\dagger Q = \frac{g^{\frac{1}{6}}w^{\frac{1}{3}}\psi\Psi^{\frac{2}{3}}}{\sqrt{2}} \cdot \frac{h^{\frac{1}{2}}R^{\frac{5}{3}}}{l^{\frac{1}{3}}\lambda_1^{\frac{1}{3}}}. \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (56)$$

$\dagger$ *Editor's Note.*—I have inserted $\lambda_1^{\frac{1}{3}}$ in this expression; it was omitted in the MS.

By equation (58),

$$\frac{2\psi}{(\Psi\gamma)^2} = \frac{2\frac{c}{b}}{\left(\frac{c}{b+c}\right)^2 \gamma^2} = \frac{2(b+c)^2}{bc\gamma^2}.$$

By equation (53),

$$Q = \frac{2(b+c)^2}{bc\gamma^2}\left\{1 - \left(\frac{bc\gamma}{b+c}+1\right)\epsilon^{-\frac{bc\gamma}{b+c}}\right\}v_0 . \quad (60)$$

Substituting in like manner b, $\frac{c}{b}$, $\frac{c}{b+c}$ for R, ψ, and Ψ in equation (55),

$$\frac{\psi}{\Psi^{\frac{5}{3}}} = \frac{\frac{c}{b}}{\left(\frac{c}{b+c}\right)^{\frac{5}{3}}} = \frac{(b+c)^{\frac{5}{3}}}{bc^{\frac{2}{3}}}.$$

Observing that

$$\frac{h^{\frac{1}{2}}}{l^{\frac{1}{3}}} = \left(\frac{h}{l}\right)^{\frac{1}{2}} l^{\frac{1}{6}} = (\sin i)^{\frac{1}{2}} l^{\frac{1}{6}},$$

$$\therefore\; Q = \frac{2^{\frac{1}{2}}g^{\frac{1}{6}}w^{\frac{1}{3}}(b+c)^{\frac{5}{3}}}{\gamma^2\lambda_1^{\frac{1}{3}}b^{\frac{2}{3}}c^{\frac{2}{3}}}\left(\epsilon^{\frac{\gamma bc}{b+c}}-\frac{\gamma bc}{b+c}-1\right)(\sin i)^{\frac{1}{2}}l^{\frac{1}{6}},$$

$$Q = \frac{20\cdot691(b+c)^{\frac{5}{3}}}{\gamma^2\lambda_1^{\frac{1}{3}}(bc)^{\frac{2}{3}}}\left(\epsilon^{\frac{\gamma bc}{b+c}}-\frac{\gamma bc}{b+c}-1\right)(\sin i)^{\frac{1}{2}}l^{\frac{1}{6}}. \quad (61)$$

Flow of water through a closed rectangular pipe placed in continuation of an open stream of greater section and fed by it.

Two sets of experiments were made by MM. Darcy and Bazin [*] on the flowing of water through closed pipes with rectangular sections, which they completely filled, the former in 1857 with a pipe having an internal section of 0·8 metre by 0·5 metre, a length of 70·3 metres, and an inclination of 0·0049 metre per metre, and the latter with one having an internal section of 0·48 metre by 0·3 metre, a length of 44·43 metres, and an inclination of 0·0049 metre. Each of the pipes was fixed in the bed of an open stream 200 metres long, of uniform section and inclination. The water could be supplied from the Canal de Bourgogne, and

* *Recherches Hydrauliques,* Paris, Imprimerie Royale, 1865, p. 162.

was carried off by the river Auche. The pipe was formed of planks of poplar nailed lengthwise on wooden frames; their joints were carefully made water-tight; and when the pipe was fixed, the space between it and the channel of the stream was filled with earth rammed down. As the water was entirely to fill the pipe, it was necessary that each end should be under water. A bar was for this purpose provided across the stream at the upper end of the pipe, so as to keep the level of the water above the mouth, and a dam was made across the stream 20 metres below the lower end of the pipe, high enough to keep the water also above the level of that end. The upper edge of this dam could be raised at will, so as to keep the lower end of the pipe (by which the efflux took place) immersed to any required depth. It was by varying the height of this dam that the quantity of water which flowed through the pipe in a given time was made to vary. The end of the pipe by which the water entered it was 129 metres from the point where it was received from the canal into the open channel. Having flowed 129 metres freely along the open channel of greater section than the pipe, it could not but have acquired a greater amount of *vis viva* than that with which the water ultimately flowed from the pipe.

Since $\dfrac{h}{\gamma}$ is the effective head of water, where h is the vertical height of the top over the bottom of the pipe, and the effective height is increased by the barrage which brings the level of the water where it enters the pipe above the top of it, it follows that γ is diminished by the barrage; and if there were no other causes operating on the other hand to increase γ, it would necessarily be less than unity. These causes have been before discussed; one of them is the accumulation during the descent of the water through the pipe of the work U_1 which it carries away with it. The other is the work U_2 expended in the horizontal pipe on the water in descending through the reservoir and in entering the pipe. But in this case the water descends through 130 metres of an open channel of much larger section than the pipe before it enters; and thus when it enters it has already acquired an amount per cubical unit at least equal to that with which it leaves it; moreover the difference between the aggregate *vis viva* of the water of the stream and that of the pipe cannot be expended wholly in causing it to boil up to the level of the barrage; part of it cannot but take effect in aiding the rush of water into the pipe, and thereby increasing the effective height, and thus, instead of increasing γ, diminishing it. As to the work U_2, that part of it which is due to the resistance to the descent of the liquid before it enters the pipe may obviously in

this case be neglected, as may that which is due to the contraction of the current in the act of entering the pipe, its section being so great as compared with its periphery.

On the whole, then, γ is to be expected to be, in this case of an inclined closed pipe fed by an open stream, less than in the case of a horizontal closed pipe fed from a reservoir; and it may be less than unity.

This will appear from the two following Tables, which have reference to experiments made with the two rectangular pipes above described, in one of which the value of γ is assumed to be 1·5, and in the other 1.

Experiments, series 52, p. 170, closed rectangular pipe, breadth $(b) = 0.48$ metre, depth $(c) = ·3$.

By equation (60),

$$Q = \frac{2(·48 + ·3)^2}{·48 \times ·3\gamma^2} \left\{ 1 - \left(\frac{·48 \times ·3\gamma}{·48 + ·3} + 1 \right) \epsilon^{-\frac{·48 \times ·3\gamma}{·48 + ·3}} \right\} v_0;$$

$$\therefore Q = \frac{8·45}{\gamma^2} \left\{ 1 - (·1846\gamma + 1)\epsilon^{-·1846\gamma} \right\} v_0.$$

$$\gamma = 1·5,$$

$$\therefore Q = \frac{8·45}{2·25} \left\{ 1 - 1·2769\,\epsilon^{-·2769} \right\} v_0,$$

$$Q = 3·77 \left\{ 1 - ·96805 \right\} v_0,$$

$$Q = ·11997225\, v_0. \quad . \quad . \quad . \quad . \quad . \quad . \quad (61)$$

Table VIII.

Experiments, Series No. 52, p. 170.   Closed rectangular pipe, 0·48 metre broad, 0·3 metre deep. (March and April 1859.)

Index number.	Velocity at axis, $v_{0'}$	Discharge per second in cubic metres, Q.	
		By theory, $\gamma = 1·5$.	By experiment.
	metre.	m. c.	m. c.
1.	0·465	0·0558	0·054
2.	0·672	0·0806	0·078
3.	0·886	0·1065	0·100
4.	1·103	0·1323	0·129
5.	1·306	0·1567	0·155
6.	1·634	0·1960	0·191
7.	1·777	0·2132	0·203
8.	1·966	0·2359	0·233

In the experiments of MM. Darcy and Bazin, Series 51, closed rectangular pipe, breadth (b) internally was 0·8 metre, and its depth (c) 0·5 metre. For this series of experiments therefore, by equation (60),

$$Q = \frac{2(\cdot8 + \cdot5)^2}{\cdot8 \times \cdot5\gamma^2} \left\{ 1 - \left(\frac{\cdot8 \times \cdot5\gamma}{\cdot8 + \cdot5} + 1 \right) \epsilon^{-\frac{\cdot8 \times \cdot5\gamma}{\cdot8 + \cdot5}} \right\} v_0;$$

$$\therefore Q = \frac{8\cdot45}{\gamma^2} \{ 1 - (\cdot3077\gamma + 1) \epsilon^{-\cdot3077\gamma} \} v_0. \quad . \quad . \quad . \quad (62)$$

Assuming $\gamma = 1$,

$$Q = 8\cdot45 \{ 1 - 1\cdot3077 \epsilon^{-\cdot3077} \} v_0,$$

$$Q = 8\cdot45 (1 - \cdot96133) v_0,$$

$$Q = \cdot32676 v_0.$$

Table IX.

Experiments, Series No. 51, p. 168.   Rectangular pipe closed. Breadth 0·8 metre, depth 0·5 metre. (October and November 1857.)

Index number.	Velocity at axis of pipe, v_0.	Discharge per second in cubic metres, Q.	
		By theory, $\gamma = 1$.	By experiment.
	metres.	m. c.	m. c.
1.	0·618	0·2019	0·203
2.	0·908	0·2967	0·307
3.	1·213	0·3964	0·411
4.	1·505	0·4918	0·515
5.	1·826	0·5967	0·618
6.	1·961	0·6408	0·674
7.	2·115	0·6912	0·721
8.	2·270	0·7418	0·777

Open Channels.

The steady flow of a Liquid in an open channel of a constant section and uniform direction and inclination.

If we imagine a plane to pass through the axis of either of the rectangular pipes above referred to and to be parallel to the bottom of the pipe, it will divide the water flowing through the

pipe into two equal portions, acted upon by similar and equal
forces, and subjected to similar and equal conditions of motion.
If, therefore, we conceive the portion of the liquid above the
plane to be removed, and the same pressures to be exerted on
every point of the surface of the portion which remains as it
exerted, it is clear that the motion of the latter will remain un-
changed, as also the discharge of that portion, which, as the
discharge of the whole was represented by Q, will be represented
by $\frac{1}{2}$Q.   But by the removal of the upper portion of the liquid,
the pressure on the different points on the surface of that which
remains will be changed; for whereas before it cannot but have
been different at different distances from the axis, because the
velocities at such different distances were different*, now they
are the same, being everywhere equal to the pressure of the at-
mosphere.   As the pressure is everywhere less where the velocity
is greater, it is evident that there will be a tendency in the liquid
on the surface to flow from the sides of the channel towards the
centre, and that thus the velocity of the surface-water at the
centre will be diminished, and the water heaped up, drowning,
as it were, the point of greatest velocity in the section.

This disturbance of the motion of the liquid will not be limited
to the surface; there will result therefore a disturbance of the
films of equal velocity.   The experiments of MM. Darcy and
Bazin afford evidence of this disturbance.   In the first of the
accompanying figures the films of equal velocity are shown in
the pipe (sect. 0·8 metre by 0·5 metre) when closed and full, and
in the second when open and half full.   (Plate I. figs. 1 & 2.).

Notwithstanding this new disturbance of the velocities of the
different parts of the liquid, it is to be observed that the work
of its weight over a given space remains the same; so that if
there is no considerable change in the resistances, the discharge
of the half of the now opened pipe may be expected to be the
same as the discharge of that half was before.

This was found by the experiments† to be the case.   The
rectangular pipes above described, whose sections were respec-
tively 0·8 metre × 0·5 metre and 0·48 metre × 0·3 metre, had
their tops removed, and water was made to flow through them
so as just to half fill them.   The following were the results:—

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* Phil. Mag. November 1871, equation (20).
† *Recherches Hydrauliques*, pp. 176 &c.

When the pipe was full and before its top was removed.		When the pipe was half full and after its top was removed.	
Section of stream	0·5 × 0·8 metre.	Section of stream	0·2458 × 0·8 met.
Slope per metre	0·00427 metre.	Slope per metre	0·0043 metre.
Discharge per sec.	0·618 m. c.	Discharge per sec.	0·307 m. c.
Section of stream	0·3 × 0·48 metre.	Section of stream	0·1513 × 0·48 m.
Slope per metre	0·00627 metre.	Slope per metre	0·006 metre.
Discharge per sec.	0·191 m. c.	Discharge per sec.	0·093 m. c.

Discharge from an open channel of any form.

If, then, Q be taken to represent the discharge from an open rectangular channel half filled, 2Q will represent that from the same channel having its section doubled and being closed and filled. If, then, we take *c*, as before, to represent the depth of the stream, and *b* its breadth, and substitute 2Q for Q in equation (60), and 2*c* for *c*, the resulting value of Q will be that of the discharge from an open rectangular channel.

Moreover the reason for the discharge from an open rectangular channel being half that from a closed one of double the section applies equally to channels of all other forms, so that the fact of the half discharge being obtained from the open channel of half the section of the closed one may be assumed to be generally true. If therefore we substitute 2Q for Q in equation (54), 2χ for χ, 2Ω for Ω, we shall obtain a formula in which Q represents the discharge from an open channel of any given form, Ω representing the section of the stream, and ψ the wetted periphery of the section of the channel. We thus obtain from equation (54) an equation for the discharge from an open rectangular channel.

By equation (60),

$$Q = \frac{(b+2c)^2}{2\gamma^2 bc} \left\{ 1 - \left(1 + \frac{2\gamma bc}{b+2c}\right) \epsilon^{-\frac{2\gamma bc}{b+2c}} \right\} v_0. \quad . \quad . \quad (63)$$

By equation (61),

$$Q = \frac{\left(\frac{g}{2}\right)^{\frac{1}{6}} w^{\frac{1}{3}} (b+2c)^{\frac{5}{3}}}{2\gamma^2 \lambda_1^{\frac{1}{3}} (bc)^{\frac{2}{3}}} \left(\epsilon^{\frac{2\gamma bc}{b+2c}} - \frac{2\gamma bc}{b+2c} - 1 \right) (\sin i)^{\frac{1}{2}} l^{\frac{1}{6}},$$

$$Q = \frac{65 \cdot 173 (b+2c)^{\frac{5}{3}}}{\gamma^2 \lambda_1^{\frac{1}{3}} (bc)^{\frac{2}{3}}} \left(\epsilon^{\frac{2\gamma bc}{b+2c}} - \frac{2\gamma bc}{b+2c} - 1 \right) (\sin i)^{\frac{1}{2}} l^{\frac{1}{6}}. \quad . \quad (64)$$

For the discharge from a uniform open channel of any given

section, inclination, or degree of roughness or smoothness,

$$Q = \frac{\chi^2}{2\Omega\gamma^2}\left\{1-\left(1+\frac{2\Omega\gamma}{\chi}\right)\epsilon^{-\frac{2\Omega\gamma}{\chi}}\right\}v_0. \quad . \quad . \quad (65)$$

In the case of an open channel $\gamma=1$. The value of γ has been shown to be dependent on that of $U_1 + U_2$*. If $U_1 + U_2 = 0$, $\gamma = 1$. Now U_2 represents the work done by the head of water to overcome the resistances to the motion of the water before it enters the pipe, and U_1 represents the work similarly done to accumulate in the water the work with which it leaves the pipe; if therefore we reason only of that portion of the liquid flowing through an open channel which is at a considerable distance from the point at which it is received into the channel, and which has acquired a uniform and steady state of motion, and if we measure the head of water at any other point below it from this point as its commencement, it is clear that the work U_1 is all done by its weight before the liquid enters upon this portion, and also the work U_2; so that the liquid enters upon this portion with no resistance of contraction any longer to be overcome, and no work further to be accumulated in it, but only with the resistances to motion in its channel and motion upon itself as it descends still to be overcome. In respect of that portion of its channel, therefore, $U_1 + U_2 = 0$ and $\gamma = 1$†.

Comparison of Theoretical with Experimental Discharge in open Channels of different shapes.

Assuming $\gamma = 1$, equations (63) and (65) become

$$Q = \left(\frac{b+2c}{2bc}\right)^2\left\{1-\left(1+\frac{2bc}{b+2c}\right)\epsilon^{-\frac{2bc}{b+2c}}\right\}v_0, \quad . \quad (66)$$

$$Q = \frac{\chi^2}{2\Omega}\left\{1-\left(1+\frac{2\Omega}{\chi}\right)\epsilon^{-\frac{2\Omega}{\chi}}\right\}v_0. \quad . \quad . \quad . \quad (67)$$

Mean velocity

$$= \frac{Q}{\Omega} = \frac{1}{2}\left(\frac{\chi}{\Omega}\right)^2\left\{1-\left(1+\frac{2\Omega}{\chi}\right)\epsilon^{-\frac{2\Omega}{\chi}}\right\}v_0. \quad . \quad (68)$$

I propose now to test these formulæ by comparing them with the important experiments made by MM. Darcy and Bazin on the motion of water in open channels. They made several hundred of such experiments in fifty series on channels of different forms and inclinations and with streams of different depths. The results of the comparison with experiments taken from among these without selection are recorded in the following Tables.

* Phil. Mag. November 1871, p. 351.

† *Editor's note.*—This is the case alluded to in the Philosophical Magazine for November 1871 as still remaining to be discussed.

TABLE X.

Open rectangular channel; the sides and bottom covered with gravel-stone fixed in cement; sides of stones of gravel from 0·01 metre to 0·02 metre in diameter. Inclination of channel 0·0049 metre per metre; breadth of channel 1·832 metre. Darcy, Series 4, pp. 75, 77.

$$Q = \frac{\chi}{\left(\frac{2\Omega}{\chi}\right)}\left\{1-\left(1+\frac{2\Omega}{\chi}\right)\epsilon^{-\frac{2\Omega}{\chi}}\right\}v_0.$$

Index number.	Depth of stream, c.	$\left(1+\frac{2\Omega}{\chi}\right)\epsilon^{-\frac{2\Omega}{\chi}}$.	Maximum velocity, v_0.	Q. By theory.	Q. By experiment.
	metre.		metres.	m. c.	m. c.
1.	0·830	0·98953	0·847	0·1164	0·100
2.	0·1234	0·98156	1·246	0·2197	0·203
3.	0·1651	0·96854	1·360	0·3357	0·307
4.	0·1917	0·95921	1·616	0·4606	0·411
5.	0·2226	0·94928	1·748	0·5636	0·515
6.	0·2499	0·93828	1·847	0·6776	0·618
7.	0·2782	0·93060	1·964*	0·7627	0·721
8.	0·3025	0·92317	2·032*	0·8365	0·824
9.	0·3240	0·91621	2·124*	0·9224	0·927
10.	0·3507	0·90760	2·205*	1·0181	1·030
11.	0·3737	0·90031	2·323	1·1253	1·133
12.	0·3967	0·89287	2·352	1·194	1·236

Note.—The maximum velocities v_0 marked with an asterisk were determined by *floats*, the others with the gauge-tube (*tube-jaugeur*).

TABLE XI.

Open rectangular channel, the sides and bottom covered with gravel-stones from 0·03 metre to 0·04 metre in diameter fixed in cement. Inclination of channel 0·0049 metre per metre; breadth of channel 1·861 metre. Darcy, Series 5, pp. 75, 76.

$$Q = \frac{\chi}{\left(\frac{2\Omega}{\chi}\right)}\left\{1-\left(1+\frac{2\Omega}{\chi}\right)\epsilon^{-\frac{2\Omega}{\chi}}\right\}v_0.$$

Index number.	Depth of stream, c.	$\left(1+\frac{2\Omega}{\chi}\right)\epsilon^{-\frac{2\Omega}{\chi}}$.	Maximum velocity, v_0.	Q. By theory.	Q. By experiment.
	metre.		metres.	m. c.	m. c.
8.	0·3436	0·90925	1·887	0·8961	0·824
9.	0·3690	0·90106	1·912	0·9305	0·927
10.	0·3946	0·89293	1·951	0·9988	1·030
11.	0·4176	0·88580	2·006	1·133	1·0716
12.	0·4448	0·87750	2·037	1·1405	1·236

TABLE XII.

Open trapezoidal channel, differing but little from a rectangle, the width at the bottom being 1·8 metre, and the sides being inclined 1 in 10; constructed with rough building-stones laid in cement. Inclination 0·037 metre per metre. The masonry of the sides was more perfect than that of the bottom, the latter being somewhat worn. The bottom was covered with a slight deposit of mud which adhered to it. Darcy, Series 33, pp. 110, 111.

$$Q = \frac{\psi}{\left(\frac{2\Omega}{\chi}\right)}\left\{1 - \left(1 + \frac{2\Omega}{\chi}\right)\epsilon^{-\frac{2\Omega}{\chi}}\right\}v_0.$$

Index number.	Depth of stream, c.	$\left(1+\frac{2\Omega}{\chi}\right)\epsilon^{-\frac{2\Omega}{\chi}}$.	Maximum velocity, v_0.	Q. By theory.	Q. By experiment.
	metre.		metres.	m. c.	m. c.
1.	0·1497	0·97176	3·49	0·7993	0·749
2.	0·2351	0·94429	4·55	1·523	1·489
3.	0·2972	0·92329	5·41	2·1861	2·247
4.	0·3553	0·90385	6·15	2·7936	2·996

TABLE XIII.

Open Trapezoidal Channel.

$$Q = \frac{\chi}{2\Omega}\left\{1 - \left(1 + \frac{2\Omega}{\chi}\right)\epsilon^{-\frac{2\Omega}{\chi}}\right\}v_0.$$

Index number.		χ.	$\frac{2\Omega}{\chi}$	$\left(1+\frac{2\Omega}{\chi}\right)\epsilon^{-\frac{2\Omega}{\chi}}$.	Maximum Velocity, v_0.	Q. By theory.	Q. By experiment.	Nature of channel.
Series.	Exp.							
					metres.	m. c.	m. c.	Trapezoidal; constructed with planks. Width at bottom 1 metre. Inclination of sides 45°. Inclination of channel 0·0015.
21.	9.	2·3038	0·58469	0·88295	1·651	0·7611	0·927	
21.	12.	2·4542	0·6739	0·81718	1·805	1·1974	1·236	
								Trapezoidal; constructed with planks. Width at bottom 0·945 metre; one of the sides inclined at 45°, the other vertical. Inclination of bottom of channel 0·0049 metre per metre.
22.	1.	1·168	0·1570	0·9889	1·420	0·1174	0·100	
22.	2.	1·2864	0·2198	0·97903	1·785	0·21907	0·203	
22.	3.	1·3893	0·2740	0·96866	1·956	0·31082	0·307	
22.	4.	1·4776	0·3150	0·95967	2·114	0·41008	0·411	
22.	5.	1·5541	0·3478	0·95187	2·390	0·4770	0·515	

TABLE XIV.

Open Channels.

$$Q=\left(\frac{b+2c}{2bc}\right)^2\left\{1-\left(1+\frac{2bc}{b+2c}\right)\epsilon^{-\frac{2bc}{b+2c}}\right\}v_0.$$

Index number		Inclination of channel, i.	Breadth of channel, b.	Depth of stream, c.	$\left(1+\frac{2bc}{b+2c}\right)\epsilon^{-\frac{2bc}{b+2c}}$	Maximum velocity, v_0.	Q.		Nature of surface.
Series.	Experiment.						By theory.	By experiment.	
		metre.	metre.	metre.		metres.	m. c.	m. c.	
2.	7.	0·0049	1·812	0·1912	·9595	2·521	·7096	·721	Cement (pure).
3.	7.	0·0049	1·911	0·2180	·95009	2·207	·728	·721	Bricks laid flat.
4.	10.	0·0049	1·832	0·3507	·90773	2·205	1·01727	1·030	Gravel; diameter 0·01 to 0·02; cemented.
5.	11.	0·0049	1·861	0·4176	·88588	2·006	1·0708	1·133	Gravel; diameter 0·03 to 0·04; cemented.
6.	1.	0·00208	1·99	0·0791	·99022	0·806	0·151	0·100	Planks.
7.	6.	0·0049	1·99	0·1851	·96148	2·216	0·6542	0·618	,,
8.	9.	0·00824	1·99	0·1976	·95624	2·847	0·9013	0·927	,,
9.	2.	0·0015	1·983	0·1414	·97398	0·928	0·221	0·203	,,
10.	3.	0·0059	1·987	0·1297	·97738	1·964	0·424	0·411	,,
11.	3.	0·00829	1·982	0·1133	·98179	2·269	0·446	0·411	,,
12.	4.	0·0015	1·960	0·3110	·91811	1·334	0·557	0·618	Planks with boards fixed across them, 0·027 metre wide and 0·01 metre apart.
13.	5.	0·0059	1·961	0·2448	·94051	2·226	0·828	0·824	
14.	6.	0·00886	1·952	0·2524	·93815	2·747	1·0407	1·030	
15.	4.	0·0015	1·960	0·4058	·88661	1·081	0·5919	0·618	The same as above, except that the boards were 0·05 metre apart.
16.	7.	0·0059	1·363	0·4202	·88187	2·060	1·1594	1·236	
17.	6.	0·00886	1·951	0·3319	·91118	2·206	1·0343	1·030	
18.	8.	0·0049	1·197	0·3339	·93061	2·560	0·77045	0·824	Planks.
19.	8.	0·0043	0·800	0·3007	·95294	2·042	0·3991	0·411	,,

The Discharge when the Maximum Velocity is not given.

The preceding Table serves to verify equation (63), in which the maximum velocity v_0 is supposed to be given by experiment, and the discharge Q determined from it. It affords no verification of equation (64), in which v_0 is not supposed to be given, but the discharge Q determined from the form and dimensions of the channel, the slope, and the nature of the surface. The difficulty of the latter verification lies in the indeterminateness of the nature of the surface as represented by $\lambda_1^{\frac{1}{3}}$.

Verification of Formula.—It may nevertheless be verified by dividing by one another the discharges from the same channel corresponding to two different depths c_1 and c_2 of the stream as given by theory and by experiment; $\lambda_1^{\frac{1}{3}}$ will be eliminated by this division from the formula representing this division. Let the discharges be represented by Q_{c_1} and Q_{c_2}; then, by equation (64),

$$\frac{Q_{c_1}}{Q_{c_2}} = \left(\frac{b+2c_1}{b+2c_2}\right)^{\frac{5}{3}}\left(\frac{c_2}{c_1}\right)^{\frac{2}{3}}\left\{\frac{\epsilon^{\frac{2bc_1}{b+2c_1}} - \frac{2bc_1}{b+2c_1} - 1}{\epsilon^{\frac{2bc_2}{b+2c_2}} - \frac{2bc_2}{b+2c_2} - 1}\right\} \quad . \quad . \quad . \quad (68)$$

Applying this formula to experiments 3 and 12, Series 2,

$$b = 1\cdot812, \quad c_1 = \cdot2773, \quad c_2 = \cdot1102;$$
$$\therefore \left(\frac{b+2c_1}{b+2c_2}\right)^{\frac{5}{3}}\left(\frac{c_2}{c_1}\right)^{\frac{2}{3}} = \cdot63535,$$
$$\epsilon^{\frac{2bc_1}{b+2c_1}} - \frac{2bc_1}{b+2c_1} - 1 = \cdot10441;$$
$$\epsilon^{\frac{2bc_2}{b+2c_2}} - \frac{2bc_2}{b+2c_2} - 1 = \cdot01898;$$

∴ by theory,

$$\frac{Q_{c_1}}{Q_{c_2}} = \cdot63535 \times \frac{\cdot10441}{\cdot01898} = 3\cdot8334;$$

by experiment,

$$\frac{Q_{c_1}}{Q_{c_2}} = \frac{1\cdot236}{\cdot307} = 4\cdot02.$$

Similarly, by comparing experiments 12 and 8, Series 2,

$$b = 1\cdot812, \quad c_1 = \cdot2773, \quad c_2 = \cdot2095,$$

we have by theory,

$$\frac{Q_{c_1}}{Q_{c_2}} = 1\cdot4702;$$

oy experiment,

$$\frac{Q_c}{Q_{c_2}} = 1\cdot 5.$$

And comparing experiment 8 and 3, Series 2,

$$b = 1\cdot 812, \quad c_1 = \cdot 2095, \quad c_2 = \cdot 1997;$$

by theory,

$$\frac{Q_{c_1}}{Q_{c_2}} = 2\cdot 6081;$$

by experiment,

$$\frac{Q_{c_1}}{Q_{c_2}} = 2\cdot 68.$$

Current-lines and Current-surfaces.

In fig. 3, A B represents a line supposed to be drawn in a horizontal plane through the point of maximum velocity at right angles to the current experimented on, MM. Darcy and Bazin's Series 53, and represented in fig. 1. The divisions along it, through which are drawn lines parallel to the sides of the channel, represent distances of 0·11 metre each; the figures represent the velocities of the current at those several points as determined by experiment. The velocity at each point is set off on the line passing through it in the direction of the current, twelve successive times; and then the points so set off are supposed to be joined across the current so as to form a series of polygons, which, if the points of division in A B had been infinitely near to one another, would have been curves. If a line of small floating bodies, such as particles of camphor, be supposed at any instant to be dropped on the surface of the current along the line A B, then at the end of 1″ these particles carried along by the water will have arranged themselves in the curve next to A B, and at the end of 2″ in the curve next beyond *that,* and so on, until at the expiration of 12″ they will form the curve *a c b.*

These current-lines are those in which particles of liquid starting at the same given instant from the same line measured across the current will *be* at the end of the same given time. If instead of a powder being dropped upon the surface along the line A B, a wavelet had instantaneously been created along that line, that wavelet would have taken the form of the current-line, and the ripple would have been created which is seen in currents. It is not necessary that such a wave should be called into existence right across the current. If created by some obstruction in the side of the channel, it would still be absorbed into the portion of the current-line nearest it and constitute an imperfect ripple.

Fig. 4 represents a vertical section through the axis of the same current, the line D C being the depth of the current in the centre, and being divided as the central line (vertically) of fig. 1 is divided.   Lines are drawn parallel to the surface through these points of division ; and the velocities at the corresponding depths, as shown in fig. 1, are set off twelve times along each of these parallel lines.   These points being joined laterally, the corresponding curves represent the positions into which the vertical filament of fluid, which coincides at any instant with D C, would be brought at the end of $1''$, $2''$, $3''$, &c. from the time of its starting. The point of maximum velocity, as shown in fig. 4, is not at the surface, but at 0·03 metre from it. This explains the lip-like form of the curve as it approaches the surface.   If we imagine a line of particles of camphor to be made instantaneously to fill the line D C, they will arrange themselves, after $1''$, $2''$, $3''$, &c., in the corresponding lines of the figure.   If instead of these particles being made to fill a vertical line, they had been made to occupy a vertical cross plane through D C, they would, after these given periods of time, have arranged themselves successively in curved surfaces, of which the current-lines of fig. 3 represent the intersections with the surface of the current, and those of fig. 4 the intersections with a vertical plane through the axis.

By equations (52) and (53) we obtain for the case of this channel

$$v_1 = 1·245 \epsilon^{-·769x}, \quad v_2 = 1·245 \epsilon^{-1·23y},$$

where v_1 is the velocity of the current measured across from the surface at a distance x from the centre, and v_2 is the velocity at a vertical depth y from the surface measured from the surface. Beneath the observed velocities in fig. 1 are given the theoretical ones as determined by these formulæ.

If i'' represent any number of seconds and each formula be multiplied by t, (tv_1) will represent an ordinate of a current-line of the surface of the liquid, corresponding to the abscissa x, t'' after it has left the position A B ; and (tv_2) will represent a similar ordinate of the current-line of the depth corresponding to the abscissa y.

Putting $(tv_1) = z_1$, and $(tv_2) = z_2$, we have

$$z_1 = (1·245t) \epsilon^{-·769x}, \quad z_2 = (1·245t) \epsilon^{-1·23y},$$

which are the equations to the current-lines of the surface and the depth respectively.

The Value of μ.

In the preceding investigation μ is taken to represent the statical resistance per unit of surface to the motion of one film

over the surface of another. As the motions of the films over one another are supposed uniform, the statical resistance must, in respect of each, equal the pressure to which the film is subjected in the direction of its motion and which causes it to move. But this last is in every case *given* in amount; the former is therefore also given in amount. It is this equivalent of μ which comes in the place of μ, and which eliminates it from the inquiry. Whence μ arises does not, therefore, come into question, but only the fact that it exists, and is of that amount without which the motion could not be uniform.

I have now brought to an end the inquiries which I proposed to myself. I have investigated the conditions of the lateral propagation of motion in a liquid flowing from a reservoir through a circular pipe, whose particles move parallel to one another with a steady motion, to be represented by the equation (see equation 13)

$$v = v_0 \epsilon^{-\gamma r},$$

where v_0 is the maximum velocity of water flowing from the axis of the pipe and γ a quantity constant for the same pipe, whatever the head of water, but variable for different dimensions of the pipe and different conditions under which it receives the water from the reservoir.

From this equation I have deduced the discharge from such a circular pipe per unit of time. It is given in terms of the maximum velocity in equation (17). The discharge being known in terms of the maximum velocity, it remained to determine the maximum velocity itself under the given conditions of the discharge. That is done by equation (29); while the absolute value of the velocity is determined by equation (28), and the discharge per $1''$ by equation (31); equations (39) and (40) determine the same thing with respect to inclined pipes. Incidental to these inquiries is that as to the *work* expended by water in flowing through a pipe, and the rise of temperature due to that expenditure of work. These are represented by equations (44) and (45).

These are the subjects of my two former papers. In the present one I have investigated the *general* conditions of the lateral propagation of motion in a liquid whose particles move parallel to one another. It results from it that the particles move in *films* geometrically similar to one another. It was the obvious fact that when a liquid moves uniformly in a circular pipe which it completely fills, its particles necessarily form themselves into such films, which was the ground of the whole of my preceding investigations. That this principle, which is true of circular pipes is true of pipes of all other forms of section, enables me to

apply the same method of investigation to them.   The filament of maximum velocity is the common axis of all the films.   Assuming the forms of the films to be given, equation (52) determines in terms of the maximum velocity the velocity at any given distance from that axis ; equation (53) determines the discharge in terms of the maximum velocity.   Equations (54a) and (54b) determine the maximum velocity itself, and equation (55) the discharge from a pipe of any given form and dimensions, the geometrical forms of whose films are also given, as they are in the case of circular pipes.

Hitherto my inquiries have applied only to the case of closed channels supplied from a reservoir, and my results have been complicated by that arbitrary constant γ which is dependent on the oblique direction of the liquid on passing from the reservoir into the channel.   In open channels this difficulty disappears, and γ assumes in every case the value unity.

The discharge from an open channel being half that from a closed channel of twice the dimensions, is determined at once from the formulæ I have given for the latter by the substitution (*mutatis mutandis*) of unity for γ.   There remains, therefore, only the determination of the relation which exists between the forms of the films and the internal form of the channel.   In the case of a closed circular channel this relation is one of identity.   In the case of channels of other forms it approaches it in greater or less degrees of accuracy.   I have tested this degree of accuracy in great numbers of the experiments of MM. Darcy and Bazin, in respect of open rectangular and trapezoidal channels of great varieties of forms and dimensions and depths of water.   The results are stated in various Tables of the present paper, and compared with the results of experiment.   It is on the faith of this comparison that I propose (in all those cases to which my comparison has extended) the following formula as representing sufficiently for all practical purposes the discharge from a stream of given section in terms of its velocity at its mid surface—that is, its maximum velocity *.   This formula is independent of the inclination of the stream, or the roughness or smoothness of the channel, both of which conditions are represented in the maximum velocity.

And now I have to acknowledge my obligations to the admi-

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* *Editor's Note.*—This formula is not actually specified in the paper. There can, however, be no doubt that it is that given in equation (67), viz.

$$Q = \frac{\chi^2}{2\Omega}\Big[1 - \Big(1 + \frac{2\Omega}{\chi}\Big)\epsilon^{-\frac{2\Omega}{\chi}}\Big]v_0,$$

where $\Omega =$ area of stream,
              $\chi =$ wetted perimeter.

rable experiments of MM. Darcy and Bazin. The labours of the late M. Darcy appear to me, as regards scientific precision, the admirable design of the instruments used in them, and the admirable industry with which they were carried on, among the greatest scientific labours of our age. I could not have arrived at my results without them; but these are not founded upon them, but on the principles of mechanical philosophy. They have served me rather as the scaffolding on which my structure has been raised than as the foundation; the scaffolding being withdrawn, the structure stands by itself.

Note referred to in page 38*.

I will venture to propose the following explanation of this formula.

The resistance to the flow of the liquid which is next to that in contact with the surface of the pipe is supposed to be due to its adhesion to the liquid fixed to the surface of the pipe and to the impinging of its molecules on those of that liquid and on those of the pipe itself. The work per unit of time of that resistance is therefore equal to the work per unit of time of the shear of a flowing liquid film over a fixed one plus the work per unit of time necessary to replace that which is lost in the impact of the molecules of the moveable film on those of the fixed one.

Let

P = whole resistance per unit of surface to the motion of the one film over the other at rest.

V = uniform velocity of the motion of the one film over the other.

K = area of the surfaces of the films in contact.

μ_1 = unit of shear, being that per unit of surface of contact of the films.

PK = whole resistance of one film to the motion over it of the other.

PKV = work per unit of time of that resistance.

$K\mu_1 V$ = work per unit of time of shear.

To determine the work lost per unit of time by the impact of the molecules of the moving film over that of the fixed one, let it be observed that this loss is proportional to the number of such impacts per unit of time and the work lost in each such impact, and that this last is measured by half the *vis viva* lost in each impact. But the number of impacts per unit of time is proportional to KV; and the *vis viva* lost in the impact of one molecule on another at rest is proportional to the square of the

* *Editor's Note.*—This note, evidently made in connexion with the present paper, was found amongst Canon Moseley's MSS.; and it seemed fitting to publish it here.

velocity of the impinging molecule. This may be proved as follows.

By a well-known formula, if W_1 and W_2 be the weights of two imperfectly elastic impinging bodies, and V_1, V_2 the velocities of their impact, and e the measure of their elasticity, and u_1 half the *vis viva* lost by W_1 in its impact, then

$$u_1 = \frac{(1+e)W_1W_2(V_1 \mp V_2)}{2g(W_1 + W_2)^2} \{2W_1V_1 + (1-e)W_2V_1 \pm (1+e)W_2V_2\}.$$

(See "Mechanical Principles of Engineering and Architecture," by the author of this paper, Art. 440.)

If $V_2 = 0$, or if one of the bodies be at rest,

$$u_1 = \frac{(1+e)W_1W_2}{2g(W_1 + W_2)^2} \{2W_1 + (1-e)W_2\}V_1^2;$$

u_1 varies, therefore, in this case as V_1^2—that is, as the square of the velocity of the impinging body.

Therefore the work lost per unit of time by the impact of the molecules of the moving film on those of the fixed one is proportional to $(KV)N^2$ or to KV^3. Let it = $\lambda_1 KV^3$;

$$\therefore PKV = K\mu_1V + \lambda_1 KV^3,$$

and

$$P = \mu_1 + \lambda_1 V^2.$$

IV. *On Variations of Pitch in Beats.* By Sedley Taylor, *Esq., late Fellow of Trinity College, Cambridge*[*].

PROFESSOR HELMHOLTZ, as all readers of his great work, *Die Lehre von den Tonempfindungen*, are aware, has proved that discord in music invariably arises from certain phenomena called *beats*, which were well known to previous writers on Acoustics, though their extreme importance had not been recognized. Beats occur whenever two musical sounds of slightly different pitch are simultaneously produced. Helmholtz describes their effect in the following words :—"The intensity of the sound becomes alternately strong and weak in regular succession." Up to the second edition of the *Tonempfindungen* no variation save that of intensity was mentioned. In the third edition, however, the author states that a slight oscillation in *pitch* on the part of the beating sound is likewise perceptible, adding that this fact was communicated to him by M. Guéroult[†]. But, although

this additional element of variation in beats is thus recognized, dissonance is still, as in the earlier editions, represented as due to alternations of intensity only. The following paper is an attempt to show that the pitch-variations are important constituents of dissonance, and ought, in a complete view of the subject, to be taken into account.

I am not aware whether M. Guéroult originated the observation the result of which he laid before Professor Helmholtz, but may remark that in a paper read before the Cambridge Philosophical Society in 1857, by the late Professor De Morgan *, the existence of alternations of pitch in beats is distinctly asserted as a known fact which he had himself experimentally verified.

It will be convenient to begin by laying down theoretically the conditions under which pitch-variation occurs, as the results thus to be obtained facilitate the experimental examination of the phenomenon.

Let two simple tones of different intensities, but of nearly the same pitch, coexist. An assigned molecule of air will be simultaneously solicited by two sets of sound-waves. Each of these may be represented by a curve of sines, the times elapsed from a given epoch being denoted by distances measured along the axis of abscissæ, and the contemporary displacements of the molecule by corresponding ordinates. The problem to be solved is the composition of the two series of vibrations. Two cases must be distinguished, according as the higher or lower of the two tones is the more powerful. We begin with the first of these.

The waves acting on the molecule may have any degree of difference of phase. The two extreme cases, where they are in complete accordance and in complete opposition, are represented in figs. 1 and 2 respectively. The strong line in each is half a wave of the resultant curve. O is the origin at the undisturbed

Fig. 1.
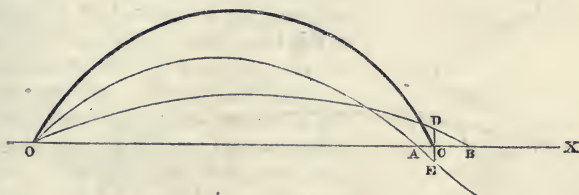
position of the molecule; O X the axis of abscissæ; O A, O B

* " On the Beats of Imperfect Consonances," Trans. of Camb. Phil. Soc. vol. x. p. 137.

half wave-lengths of the constituents ; C the point in O X where
the ordinates C D and C E are equal in length but opposite in

Fig. 2.

direction, and where, accordingly, the resultant curve meets the
axis.

In fig. 1 O C is greater than O A and less than O B, while in
fig. 2 O C is less than either O A or O B.   But O C is in each
case the half wave-length of the resultant tone, O A and O B
those of the primary tones.   Hence, when the two sets of waves
are in complete accordance, and the intensity of the resultant
tone therefore a maximum, its pitch will lie *between the two pri-*
maries.   In the opposite case, where the intensity is a minimum,
the pitch of the resultant tone will be *more acute than the higher*
of the primaries.

Figs. 3 and 4 represent the state of things when the lower of
the two original tones is the more powerful.

Fig. 3.
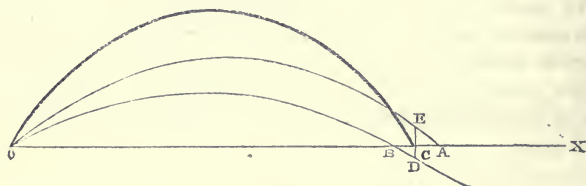
Fig. 4.
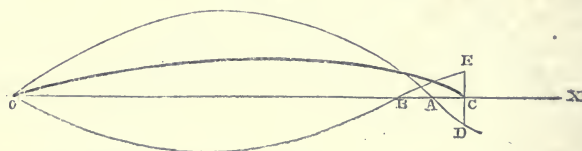
Here, when the intensity is a maximum, the pitch will be, as
in the first case, *intermediate between the primaries* ; when a mi-
nimum, *lower than the more grave of the primaries.*

Thus, in both the cases under consideration, each beat will be

of two different degrees of pitch at the moments when it is loudest and weakest respectively. In order to ascertain what happens at intermediate points of time, we have only to determine the resultant curve for a sufficient number of wave-lengths. This is done in fig. 5, which is constructed for the interval of a semitone (16 : 15), and represents completely the state of things from a maximum of intensity to the adjacent minimum. The higher tone is taken as the louder of the two.

The wave-lengths here gradually diminish as we advance from the maximum to the minimum of intensity ; and, accordingly, the pitch of the varying tone rises continuously, passing through every gradation which separates its gravest value, at the maximum of intensity, from its acutest value, at the minimum of intensity. After the latter moment the same interval is again traversed in the same manner, but in the opposite direction, the pitch falling continuously, until, at the next maximum of intensity, it once more reaches its lower limit. Thus in every beat there is a regular oscillation of pitch, as well as of intensity, each taking place between fixed limits. When, as in the case represented by the figure, the more acute primary is the louder of the two, the variable tone sinks in pitch as its power strengthens, and rises in pitch as its power weakens. When the lower primary is the louder the reverse is the case, as can be shown by a suitable figure. The effects of these two kinds of beats may be indicated in musical notation thus,
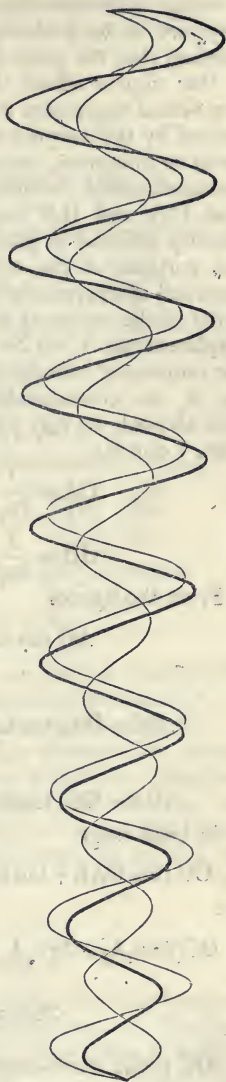
Figure 5.
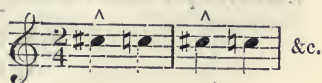
it being understood that the variations, both of pitch and intensity, are to be perfectly continuous between their limits, and, further, that the pitch-limits do not form an exact semitone.

Our figures afford the means of obtaining an approximate algebraical expression for the number of vibrations executed per second by the varying tone when its intensity is either a maximum or minimum, assuming its pitch at those moments to be instantaneously stationary. For this purpose we will assume that O A' and O B are nearly equal, *i. e.* the primaries only slightly different in pitch, and that, in determining the point C, the portions of the constituent curves about A and B may be regarded as approximately straight lines coincident with the tangents to the curves at those points. If, now, we take for the two displacements $A_1 \sin 2\pi n_1 t$ and $A_2 \sin 2\pi n_2 t$, where n_1 and n_2 are the number of vibrations per second made by the primaries, and A_1, A_2 the corresponding amplitudes, t being, as usual, the time elapsed, we may put, for the position of maximum intensity (figs. 1 and 3.),

$$OA = \frac{1}{2n_1}, \quad \tan CAE = 2\pi n_1 A_1;$$

$$OB = \frac{1}{2n_2}, \quad \tan CBD = 2\pi n_2 A_2.$$

From the figures

$$AC \tan CAE = BC \tan CBD;$$

i. e. :—

Fig. 1.

$$(OC - OA) \tan CAE = (OB - OC) \tan CBD;$$

Fig. 3.

$$(OA - OC) \tan CAE = (OC - OB) \tan CBD;$$

∴ in both cases

$$OC (\tan CAE + \tan CBD) = OA \tan CAE + OB \tan CBD,$$

i. e.

$$OC(2\pi n_1 A_1 + 2\pi n_2 A_2) = \frac{1}{2n_1} . 2\pi n_1 A_1 + \frac{1}{2n_2} . 2\pi n_1 A_2;$$

$$2OC = \frac{A_1 + A_2}{n_1 A_1 + n_2 A_2}.$$

2OC is the wave-length of the varying tone; its number of vibrations per second is therefore equal to $\frac{1}{2OC}$, or

$$\frac{n_1 A_1 + n_2 A_2}{A_1 + A_2}.$$

To find the corresponding number for the minimum position (figs. 2 and 4), it is only necessary to change the sign of one of the amplitudes; the result therefore is

$$\frac{n_1 A_1 - n_2 A_2}{A_1 - A_2},$$

A_1 being supposed $> A_2$.

The two principal cases are therefore the following :—

I. Higher tone the louder, $A_1 > A_2$, $n_1 > n_2$.

For maximum intensity, number of vibrations per second

$$= \frac{n_1 A_1 + n_2 A_2}{A_1 + A_2} = n_2 + \frac{A_1(n_1 - n_2)}{A_1 + A_2} = n_1 - \frac{A_2(n_1 - n_2)}{A_1 + A_2}.$$

For minimum intensity, number of vibrations per second

$$= \frac{n_1 A_1 - n_2 A_2}{A_1 - A_2} = n_1 + \frac{A_2(n_1 - n_2)}{A_1 - A_2}.$$

II. Lower tone the louder, $A_2 > A_1$, $n_1 > n_2$.

For maximum intensity, number of vibrations per second

$$= \frac{n_1 A_1 + n_2 A_2}{A_1 + A_2} = n_2 + \frac{A_1(n_1 - n_2)}{A_1 + A_2} = n_1 - \frac{A_2(n_1 - n_2)}{A_1 + A_2}.$$

For minimum intensity, number of vibrations per second

$$= \frac{n_2 A_2 - n_1 A_1}{A_2 - A_1} = n_2 - \frac{A_1(n_1 - n_2)}{A_2 - A_1}.$$

These equations, which embody the conclusions already obtained directly from the figures, are deduced by Helmholtz[*], as particular cases, from a more general analytical investigation.

It will be observed that when $A_1 = A_2$ a discontinuity arises, which indicates that the assumptions on which our equations were obtained are here no longer applicable. The case has not been considered by Helmholtz, but admits of easy direct treatment. The displacement of the molecule depends on the expression

$$\sin 2\pi n_1 t + \sin 2\pi n_2 t,$$

or

$$2 \sin (n_1 + n_2)\pi t \cos (n_1 - n_2)\pi t.$$

The second factor, $\cos (n_1 - n_2)\pi t$, is a slowly varying function of t which determines the number of beats per second. The first factor vanishes whenever $(n_1 + n_2)\pi t$ is a multiple of π, *i. e.* whenever t is a multiple of $\dfrac{1}{n_1 + n_2}$. This result is not, like the previous one, merely approximate, but rigorous. The successive

wave-lengths will therefore be all equal, each having the value $\frac{2}{n_1+n_2}$. Hence, in this case, there will be no variation of pitch whatever, the number of vibrations per second remaining constant, and equal to $\frac{n_1+n_2}{2}$

In passing to the experimental verification of the above theoretical conclusions, I may mention a confirmatory instance which is interesting from its undesigned character. Helmholtz has given* a copy of a curve representing the movement of a membrane resonating under the action of two beating organ-pipes, drawn by means of König's phonautograph. In this instrument a small steel pointer is connected with the membrane and registers the vibrations of the latter on a rotating cylinder which has previously been covered with lampblack-coated paper. The figure is intended by Helmholtz to illustrate variations of intensity alone; nevertheless the roughest measurement suffices to show that the wave-lengths near the minimum of intensity are shorter than those near the maximum, and therefore that pitch-variations are also present. It is clear, by what has been shown in the present paper, that in the experiment thus recorded the more acute of the two organ sounds must have been likewise the louder.

As another example of how easy it is to miss a phenomenon lying directly under our eyes, when we are not specially on the look-out for it, I may be allowed to relate a conversation with an organ- and pianoforte-tunist of much experience, Mr. Ling, of Cambridge. I asked him whether he had ever noticed changes of pitch in slow beats. He replied in the negative, and expressed a decided opinion that no such changes occurred. On trying the experiment, however, upon a pianoforte in my rooms, he at once perceived the variations in question, though remarking that he would not have accepted the fact save on the testimony of his own ears, so convinced was he that the alternations of intensity, to which he had hitherto exclusively attended, constituted the whole of the phenomenon.

I had afterwards the advantage of Mr. Ling's assistance in some experiments made on the organ in the chapel of Trinity College. A stopped diapason, A♭, having been flattened until it gave slow beats with the neighbouring G, we found, in accordance with the result obtained above for sounds of equal intensity, that the pitch of the beating tone was perfectly steady. On combining, however, the same G with an A♭ belonging to a softer stop on another manual, which had first been similarly

* P. 261.

treated, we recognized the oscillations of pitch without difficulty. By arrangements of this kind the *intermediateness*** of the beating sound between the primaries at its maximum of intensity, and its passage *beyond their limits***, either of graveness or acuteness, at its minimum, were brought out very distinctly.

Dissonance arises, as Helmholtz has proved, from the presence of beats too rapid to be separately recognized. Except when they are due to sounds of exactly equal intensities, they contain, as we have seen, oscillations of pitch. It remains to inquire whether alternations of this latter kind produce any appreciable effect on the character of the combination in which they occur. There seems some antecedent probability that they will do so. The essential character of a musical sound is steadiness of pitch. Here we have a sound which is never steady for a moment, and thus comes under the definition of a mere noise. The unmusical nature of such a sound can hardly fail to make itself felt. We can, however, put the question to the test of experiment by producing rapid alternations of intensity, while the pitch is kept constant, and ascertaining whether the whole sensation we call dissonance can be thus excited. In order to do this, I employed the principle of interference, as exhibited by a single tuning-fork. It is known that a fork in making one revolution about its own axis passes through four positions of maximum intensity separated by four positions of absolute silence. By twirling the handle of the fork between the fingers of both hands, I was able to command quick alternations of intensity unaccompanied by changes of pitch. As a further experiment, a resonance-box, with its fork screwed into it, was fastened into a lathe, and, the fork having been struck, set rotating, so that the open and closed ends of the box were alternately presented to the ear. The lathe was then urged to its fullest speed and a series of very rapid intensity-variations produced. I owe the means of performing this experiment to an excellent amateur turner, Mr. J. Aspinall, of Crewe, who was kind enough to place his lathe and his own dexterity at my disposal for the purpose. The result of both the trials just described was, that the effect, though distinctly *rough*, lacked the peculiar *sourness* usually characteristic of a discordant interval. These observations seem to show that it is the pitch-variations which excite in our ears the special element of discord just mentioned. In fact the conditions under which dissonance arises frequently themselves indicate that variations of intensity can have but an insignificant influence upon the total result. Thus the dissonance of two tuning-forks forming the interval of an

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* These facts were also experimentally ascertained by Helmholtz, p. 622.

imperfect octave is due to the beats of the combination-tone of the first order with the lower of the two fork-tones.   The former sound is much weaker than the latter ; the variations of intensity cannot, therefore, be considerable, though the sourness of effect is marked.   A similar observation applies to the case of two simple tones forming the interval of an imperfect fifth, where the dissonance arises between combination-tones of the first and second orders, the latter being extremely weak compared with the former.   Further, with composite sounds, such as those of most musical instruments, the dissonance of all intervals wider than a tone, or tone and a half, is due to the beats of over-tones of different orders.   These are, in general, correspondingly different in loudness ; and, accordingly, variations of pitch are developed at the expense of those of intensity.   I should, in particular, attribute the slight imperfections by which the ordinary concords of the scale (fifth, fourth, thirds, &c.) are rendered less smooth than the unison or octave, mainly to the pitch-variations, since the difference in order, and consequently in strength, between the beating over-tones is here usually sufficient to render the intensity-variations inconsiderable.

V. *On* Mr. Moon's *views on Gaseous Pressure.*   *By* the Hon. J. W. Strutt, *M.A., late Fellow of Trinity College, Cambridge.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

I WISH to make a few remarks on some views of Mr. R. Moon promulgated in your June Number and (more at length) in the Number for August 1868.   It is a received opinion among physicists that, in the case of such motions of air as constitute sound, the differential pressure is proportional to the differential density.   Mr. Moon, on the contrary, holds that density (and temperature) do not determine pressure, and brings forward a mathematical argument (which I shall consider presently) to show that velocity also is a datum which it is necessary to know before pressure can be calculated.   The first question that arises is, What is here meant by velocity ?   If it be *absolute* velocity which is intended, two repetitions of Boyle's (*not* Mariotte's) experiment with an interval between of twelve hours during which the earth's diurnal motion at the place of observation would be reversed, would suffice to settle the question. Perhaps Mr. Moon means the velocity relatively to the containing vessel ; but this does not alter the matter, because in the case chosen for illustration the cylinder may be moved in the direction of its length with any velocity, and leave the air behind

it; for no account is taken of friction or viscosity. But apart from all this, is it not obvious that the physical condition of a small mass of air is independent of any velocity animating all its parts? If the pressure changes, why not the other properties of air with it?

With regard to Mr. Moon's analytical argument, I would remark that the question at issue is a purely physical one. Apart from their meanings, it would no doubt be difficult to establish the proportionality of the symbols p and ρ. But Mr. Moon's reasoning is fallacious. In any particular case of motion in one dimension, p, v, and ρ are doubtless functions of x and t. Eliminate x and t, says Mr. Moon, and you are left with p, a function of v as well as ρ. In this expression lurks an ambiguity. Mathematicians often say that one quantity is a function of a second, meaning that they *may* depend on one another, without exclusion of the particular case of independence. If Mr. Moon objects to this use of the word function, his inference that p is a function of v does not follow. His opponents, of course, maintain that when you eliminate x and t, v will disappear with them; but they do not arrive at this conclusion, as Mr. Moon seems to suppose (Phil. Mag. vol. xxxvi. p. 117), as the result of a process similar to that adopted by him. Whether an assumption is gigantic or not depends on the grounds on which it is made; and I have never heard that Boyle's law was regarded otherwise than as a clear result of experiment. It is a part of that result that the velocity of the containing vessel (which is shared by the gas) is not an element in the matter.

If you begin by throwing overboard the relations between symbols which exist in virtue of their physical interpretation, you need not be surprised if an analytical process fails to force them back on you.

I am, Gentlemen,

Your obedient Servant,

JOHN W. STRUTT.

VI. *On a Bicyclic Chuck.* By A. CAYLEY, *F.R.S.*[*]

I RETAIN the former title; but I wish chiefly to describe the general plan of a curve-tracing apparatus. The arrangement may be considered as consisting of two chucks, X, Y, capable of connexion in any manner, and of a pentagraph working from the underside of a bridge, the pencil P on the upper surface of chuck X, or, say, on the chuck-table X, and the pencil Q on the chuck-table Y, each pencil being capable of attachment to the corresponding chuck-table.

* Communicated by the Author.

Then, the chucks being connected and Q being attached to Y—moving X, this moves Y, which moves Q, which (through the
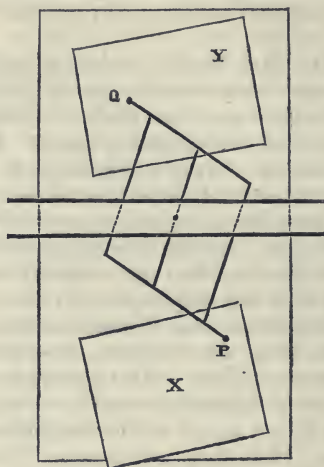
pentagraph) moves P; that is, we have the chuck-table X and the pencil P both of them in motion, and P traces out on X a curve compounded of these two motions.

In my own roughly constructed apparatus, X is a bicyclic (or an oval) chuck, Y a mere circular chuck rotating through an angle of about 40°; and a point M of the chuck-table X can be so connected as to be at a given distance from a point N of the chuck-table Y; the complete rotation of X thus gives to Y a reciprocating circular motion. The connexion is made by an excentric working below the chuck-table X; viz. we have rigidly connected with the chuck X a cylindrical axis of about 3 inches radius and ¼ inch high, carrying rigidly connected therewith the chuck-table X; and the excentric works round this axis (the centre of the axis being thus the point M), and its other extremity round an axis on the upper surface of chuck-table Y: the pencil Q can be connected (within limits) with any point of the chuck-table Y.

The result is that, the chucks being disconnected, the fixed pencil P traces out on chuck-table X the curves determined by the construction of this (bicyclic or oval) chuck; but when the connexion is established, the complete rotation of X gives to Y a reciprocating circular motion, which Y communicates to Q, and Q to P; that is, the pencil P, instead of being fixed, has a reciprocating circular motion. I obtain hereby, among other forms, elegant three-looped curves.

Suppose the chucks disconnected and the chuck-table Y fixed; then by placing Q on a determinate point of Y, we thereby place P in the position to trace out (on the moving chuck-table X) a curve of determinate form. For instance, the bicyclic chuck X being adjusted in any given manner, it is easy to draw on the fixed chuck-table Y a locus which is such that, Q being placed at any point thereof, the curve traced out by P shall be cuspidal (the locus in question is, with a difference of position only, the locus of P such that the curve traced out by P shall be cuspidal); and this being so, as Q is placed on the one or the other side of the locus, the curve traced out by P will have or will not have a node. The locus is of a high order—in the case in which I have drawn it, a closed self-intersecting curve met by various lines in four real points. Moving Q along any such line, the curve traced out by P is successively A, C, N, C, A, C, N, C, A (A without a node, C cuspidal, N nodal). Similarly, if Q be moved along a line which meets the locus in only two real points, then the forms are A, C, N, C, A; and if the line does not meet the locus, then the form is A throughout. We thus by means of the locus obtain a curve of the required form.

Again, supposing the chucks disconnected, and Q attached to Y: keeping Y fixed and moving X we obtain on chuck-table X a curve; moving Y through a small interval and again fixing it, we obtain a second curve, and so on; that is, we obtain on chuck-table X a series of curves each due to the construction of X, but in an arrangement due to the construction of Y.

VII. *Notices respecting New Books.*

An Introductory Algebra, containing the chief rules in the first part of Colenso's Elements of Algebra simplified; with additional illustrations, and followed by a large Appendix of new examples arranged in the order of the rules. By the Right Rev. J. W. Colenso, *D.D., Lord Bishop of Natal, and the* Rev. J. Hunter, *M.A., formerly Vice-Principal of the National Society's Training College, Battersea.* London: Longmans, Green, and Co. 1872. 18mo. Pp. 242.

THE title-page, which we have given at full length, sufficiently describes the work. It contains the usual course of Elementary Algebra, including, however, indeterminate equations of the first order connecting two unknown quantities, and excluding the Binomial Theorem. All elementary books on Algebra with which we are acquainted place the solution of simple equations early in the course; the present authors go a step further in this direction than most of their predecessors; for no sooner has the learner been taken through addition and subtraction than they set him to work easy equations of the first degree of one unknown quantity, then to express numerical relations in algebraical language; and thus they lead

him up to easy problems producing simple equations. For instance, such a question as the following occurs very early in the book :—
" A and B began to play with equal sums ; A won 30 shillings ; and then 7 times A's money was equal to 13 times B's : what had each at first ? " But before letting the learner try such questions, he is set a number of exercises like the following :—If x stand for a number of shillings, and if A has 13 shillings and B has 21 shillings, (1) what will A have after spending x shillings ? (2) What will B have if he receive twice as many shillings as A has spent ? (3) If A receives x shillings from B, how many will each have ? and so on. On the whole the book seems very well done ; and the large number of examples which it contains will doubtless make it useful to learners, who are at first mainly concerned to acquire a sort of mechanical ease in manipulating algebraical expressions.

A Treatise on Attractions, Laplace's Functions, and the Figure of the Earth. - *By* John H. Pratt, *M.A., F.R.S., Archdeacon of Calcutta.* Fourth edition. London and New York: Macmillan and Co. 1871. Crown 8vo. Pp. 245.

The third edition of this book was the subject of a rather long notice in our pages (vol. xxxi. pp. 144–149) ; we shall therefore limit our notice of the present edition to making mention of the chief points in which it differs from its predecessor. These are three in number :—(1) The chapter which treats of the Attraction of Table-lands, Mountains, &c. has been greatly enlarged. The author has introduced a discussion of certain formulæ for calculating the effect of variations in the density of the earth's superficial mass, which formed the subject of a pamphlet written by him in 1868. (2) The chapter which treats of the Earth considered as a Fluid Mass has been modified to bring it into accordance with the author's recognition of the fact that the variations of the force of Gravity on the Earth's surface are not in themselves sufficient to prove that the earth was originally fluid. (3) In the section on the determination of the mean figure of the earth, supposing it to be spheroidal, the author gives at much greater length than in the earlier edition his views of the best method of deducing the mean value of the Axes of the Earth from the principal measured long arcs, viz. the Anglo-Gallic Arc, the Russian Arc, and the Indian Arc. The substance of this addition appeared in our pages (vol. xxxiii. p. 10). Besides these there are several minor alterations, the result of the whole being to increase the volume by 83 pages. The preface is dated Calcutta, November 8, 1871, not long before the author's death ; this date, compared with that of the second edition of the author's Treatise on the Mathematical Principles of Mechanical Philosophy (October 5, 1841), shows that his life in India extended over more than thirty years ; and when we remember that his scientific pursuits were entirely additional to his professional duties, that they were pursued under the disadvantage of the climate of Calcutta, involved laborious calculations, and were continued up to the end of his life, we cannot but regard the composition of the volume before us as a striking instance of devotion to science.

VIII. *Proceedings of Learned Societies.*

ROYAL INSTITUTION OF GREAT BRITAIN.

May 3, 1872. ON Optical Phenomena produced by Crystals submitted to Circularly Polarized Light. By William Spottiswoode, Esq., LL.D., M.A., Treas. R.S. and R.I.

On a former occasion[*] I exhibited some phenomena depending upon circular, or as it was then also called, successive polarizaon, and in particular I adopted and explained a method for producing circularly polarized light devised by Sir Charles Wheatstone. I propose on the present occasion to pursue the subject into some of its ulterior consequences. In terms of the wave theory, light is said to be circularly polarized when the vibrations are circular, as distinguished from plane polarization (when they are rectilinear). And further, it is known from mechanical principles that a circular vibration may always be produced by the combination of two rectilinear vibrations, the amplitudes or extents of which are equal, and whereof one is in advance or in rear of the other by one or by any odd number of quarter wave-lengths. In the former of these cases the circular motion will take place in one direction, say right-handed ; in the latter in the opposite, say left-handed. The contrivance used for producing circular polarization this evening is known by the name of a "quarter-undulation plate," and consists of a plate of mica split to such a thickness that one of the two rays into which plane-polarized light is divided on entering it is retarded by an odd number of quarter wave-lengths behind the other.

The optical phenomena produced by crystals when submitted to polarized light are usually divided into two classes, viz.:— (1) those arising from the use of parallel light, and consisting of broad sheets of colour ; and (2) those due to convergent light, and consisting of the rings and brushes, the general character of which is well known. I propose to take a few specimens from each class, and to examine the modifications which the known phenomena undergo when the light is both polarized and analyzed circularly, *i. e.* when one quarter-undulation plate is interposed between the polarizer (Nicol's prism) and the crystal to be examined, and the second between the crystal and the analyzer (Nicol's prism).

In the first place, it is known that if a plate of selenite be placed in an ordinary apparatus when the polarizer and analyzer are either parallel or crossed, there are four positions at 90° apart in which the plate will produce colour—and further, that if the analyzer be turned through 90° the same result will be obtained, except that the colour will be complementary to that first seen. The intensity of the light at any given point is then given by the formula

$$\cos^2 s - \sin 2i \sin 2(i-s) \sin^2 \frac{\theta}{2},$$

where i and s are the angles made with the original plane of polar-

ization by the principal sections of the crystal and of the analyzer respectively, and θ is the retardation.

If, however, the two quarter-undulation plates (say the plates A and B) be introduced, the light undergoes the following processes :— First, it is plane-polarized by the polarizer ; secondly, the plate A being placed so that its axis is inclined at $+45°$ to the original plane of polarization, the light undergoes right- or left-handed circular polarization, and in that condition falls upon the crystal ; thirdly, in their passage through the crystal C the rays are each divided into two, whose vibrations are at right angles to one another, and whereof one is retarded in proportion to the thickness of C; fourthly, the plate B being placed so that its axis is parallel or perpendicular to that of A, each of these sets of rays is circularly polarized, one set right-handed and the other left-handed ; fifthly, these two oppositely circularly polarized sets of rays combine, according to known mechanical laws, on emerging from B into plane rays, in which the planes of polarization of the different colours of the spectrum are turned through different angles. Hence, finally, by turning the analyzer round we shall cross these various planes in turn and successively extinguish the different colours, leaving the complementary colours visible. The system of plates A, C, B consequently acts in this respect like quartz. It is, however, to be observed that if the plate B be turned from one of the two proposed positions to the other, the directions of motion in the two emergent circularly polarized rays, and consequently the planes of polarization of the different colours, will be reversed ; in other words, with the plate B in one position we shall imitate a right-handed, with the plate B in the other a left-handed quartz. The intensity of the light at any point is then given by the formula

$$\sin^2\frac{\theta}{2} \text{ for one position,}$$

$$\cos^2\frac{\theta}{2} \text{ for the other.}$$

Again if, the plates A B retaining either of the positions before indicated, the crystal C be turned round in its own plane, then, since the light emerging from A and B is circularly polarized, it has lost all trace of direction with reference to the positions of the polarizer and analyzer, and consequently no change of tint will be observed. The same is abundantly clear from the formula written above, because the only term it contains depends upon the retardation within the crystal C. This experiment was made by Airy.

If the plates A and B have their axes directed 45° on either side of the axis of C, and the three plates be turned round as one piece, the colour will remain unchanged ; while, if the analyzer be turned, we have the colours shown in the regular order. If the plates A and B have their axes directed at 45° on the same side of the axis of C, and the pieces be turned round bodily as before, the colours change in the same order as above, and go through their cycle once in every

90° of rotation ; and if the analyzer be turned in the same direction, the colours change, but in the reverse order. The explanation of this is to be found in the fact that when the plates A and B are crossed, the retardation due by A is compensated by that due to B ; so that the only effective retardation is that due to the crystal C. But upon this depends the rotation of the plane of polarization ; if, therefore, the polarizer and analyzer remain fixed, the colour will remain unaltered. When the plates A and B have their axes parallel there is no compensation, and the colour will consequently change. This experiment was made by Fresnel. The mathematical expressions for the intensity of the light in the two cases respectively are

$$\cos^2\left(j+i+\frac{\theta}{2}\right), \text{ and } \cos^2\left(j-i-\frac{\theta}{2}\right),$$

where i is the angle made by the principal sections of A with that of the polarizer, and j that of the principal section of B with that of the analyzer. The first expression is obviously unchanged when the angle between the polarizer and analyzer, viz. $\frac{\pi}{2}+i+j$, is unchanged.

It should be added that the rotation of the plane of polarization, and consequently also the sequence of tints, does not follow exactly the same law in the above cases as in quartz.

We now come to the case of convergent light—that is, to the phenomena of crystal rings ; and let us examine the effects produced by the same arrangement as before, viz. two quarter-undulation plates A, B, one in front and one behind the crystal C. To quote from Mr. Airy :—" The first thing that strikes us in this combination is that there is nothing, except in the crystal, that has any respect to sides. For the only incident light is circularly polarized ; the only light allowed to emerge is circularly polarized. The appearance therefore of the coloured rings &c. must be such as conveys no trace of any plane of polarization, and must not vary as the crystal is turned round. In the common exhibition of the coloured rings the principal trace of the planes of polarization is in the uncoloured brushes. In uniaxial crystals they form an eight-rayed star, composed of two square crosses, inclined at any angle equal to that between the planes of polarization, every ray of which separates complementary rings. In biaxial crystals they compose two pairs of rectangular hyperbolas, the angle between whose asymptotes is the same as that between the planes of polarization, and whose branches divide complementary rings. The two crosses or two sets of hyperbolas unite when the planes of polarization are parallel or perpendicular. But in the case under consideration the rings exhibited by crystals will not be traversed by any brushes. Uniaxial crystals will exhibit circular rings without a cross ; and biaxial crystals will exhibit complete lemniscates, without any interruption from curved brushes." And it is further to be noticed, as the formula given above indicates, that the centres of the rings will be bright or dark according as the analyzer stands at 0° or 90°.

To pursue this matter further. Suppose that, the arrangements remaining otherwise as before, the analyzer be turned round ; then in any position intermediate to 0° and 90° the rings will be contracted and extended in opposite quadrants, until at 45° they are divided by two diagonals, on each side of which the colours are complementary. Beyond 45° the rings begin to coalesce, until at 90° the four quadrants coincide again. During this movement the centre has changed from bright to dark. If the motion of the analyzer be reversed, the quadrants which before contracted, now expand, and *vice versâ*. Again, if the crystal (say positive) be replaced by another (say negative), the effect on the quadrants of the rings will be reversed. This method of examination therefore affords a test of the character, positive or negative, of a crystal.

A similar process applies to biaxial crystals ; but in this case the diagonals interrupting the rings are replaced by a pair of rectangular hyperbolas, on either side of which the rings expand or contract ; and the effect is reversed either by reversing the motion of the analyzer, or by replacing a positive by a negative crystal, or *vice versâ*. The experiment may then be made in biaxial crystals by turning the analyzer slightly to the right or to the left, and observing whether the rings advance towards, or recede from, one another in the centre of the field. In particular, if, polarizer and analyzer being parallel, the plate A have its axis in a N.E. direction to a person looking through the analyzer, the plate B its axis in a N.W. direction, and the crystal be so placed that the line joining the optic axes be N.S., then on turning the analyzer to the right the rings will advance to one another if the crystal be negative, and recede if it be positive. The mathematical expression for the intensity of the light at any point P is in this case

$$\tfrac{1}{2}(1 + \sin 2j \cos \theta + \sin 2b \cos 2j \sin \theta),$$

where b is the angle between the principal section of C through P and the principal section of B, and j the angle between the principal sections of B and the analyzer. This shows that when the polarizer and analyzer are parallel or crossed at 0° or 90°, and consequently $j = 45°$ or 135°, the expression is independent of b (*i. e.* the intensity is the same throughout circles about the centre), but that when the polarizer and analyzer are crossed we have an expression of the form

$$\tfrac{1}{2}(1 \pm \sin 2b \sin \theta),$$

the sign of the second term depending upon the direction in which the analyzer has been turned, and also upon the sign of θ—that is, upon the character (positive or negative) of the crystal.

The dispersion of the planes of polarization effected by the passage of plane-polarized light through a plate of quartz cut perpendicular to the axis may be rendered visible by interposing such a plate of quartz between the polarizer and a uniaxial or biaxial crystal when the analyzer is at 90°, *i. e.* when dark brushes are formed. In this case the brushes cease to be black and are tinged throughout with colour. The analyzer, however, must be turned back or forward, according as the quartz be right-handed or left-handed, in order that

it may cross in succession the planes of polarization of the different coloured rays, and so produce the most vivid effects. The dispersion of the brushes by a plate of quartz, however, may be studied by employing an additional polarizer and quartz plate between the source of light and the whole system previously used. By turning this polarizer round we extinguish each ray of the spectrum in turn and tint the whole field with the complementary colour. The brushes will then appear to revolve about their centres as the tints vary continuously from one end of the spectrum to the other. If the polarizer be turned still further round, the tints which had changed continuosly from red to violet, or *vice versâ*, change suddenly from violet to red, or *vice versâ*, and the brushes jump suddenly back to their original position.

This last optical arrangement may be employed to examine the more important phenomena of the dispersion of the optic axes produced, not by a quartz plate between the usual polarizer and crystal, but by certain biaxial crystals themselves.

ROYAL SOCIETY.

[Continued from vol. xliii. p. 542.]

March 14, 1872.—The Earl of Rosse, D.C.L., Vice-President, in the Chair.

The following communication was read :—

" The Decomposition of Water by Zinc in conjunction with a more Negative Metal." By J. H. Gladstone, Ph.D., F.R.S., and Alfred Tribe, F.C.S.

Pure zinc is incapable of decomposing pure water, even at 100° C., but at a considerably higher temperature it is known to combine with its oxygen. Davy exposed pure water for two days to the action of a pile of silver and zinc plates, separated only by pasteboard, without obtaining any hydrogen; Buff, however, has shown that a very minute trace of gas can be formed at the ordinary temperature by a pair of zinc and platinum plates.

During a series of experiments, of which we have already published an instalment, it occurred to us to ascertain whether by bringing the two metals closer together, and so increasing the electrical tension of the liquid, we could effect the same combination of zinc with oxygen at the ordinary temperature which takes place without the second metal at a very high temperature. Thin sheets of zinc and copper were hammered together and placed in a bottle filled with distilled water. Small bubbles of gas were formed. The experiment, however, was tried in a more perfect form. Some zinc-foil was allowed to remain in a somewhat dilute solution of copper sulphate until its surface was well covered with spongy copper. The metals were thoroughly washed with distilled water, and then they were immersed in a bottle of distilled water with a delivery-tube. Minute bubbles of gas quickly made their appearance, which proved to be hydrogen, and zinc-oxide was formed. Two experiments were made quantita-

tively, the gas being collected and measured at the end of 24 or 48 hours. The quantity of gas in cubic centimetres is given in the third and fourth columns of the subjoined Table, corrected for temperature and pressure. The mean temperature in the second column is simply the mean of the maximum and minimum during the period. In experiment A, 33·4 grms. of zinc-foil were employed, being 2·6 metres long and 0·05 wide. The coils were kept apart by muslin. In experiment B there was used 1 metre of similar foil crumpled up.

Day.	Mean temp.	Exper. A.	Exper. B.	Day.	Mean temp.	Exper. A.	Exper. B.
	C.	c. c.	c. c.		C.	c. c.	c. c.
1.	12·8°	117·1	49·6	18.	6·7°	20·0	7·6
2.	12·2	93·8	37·5	19, 20.	6·1	17·2 (×2)	5·7 (×2)
3.	11·7	73·8	27·6	21.	4·4	20·0	6·6
4.	11·1	66·2	24·7	22.	5·0	15·3	4·8
5, 6.	10·0	49·3 (×2)	17·5 (×2)	Interval.			
7.	8·9	41·1	14·9	44.	10·0	20·5	5·5
8.	10·5	40·9	15·8	45, 46.	10·5	22·5 (×2)	6·5 (×2)
9.	10·0	40·9	14·8	47.	11·1	22·3	6·5
10.	7·8	33·8	10·3	48.	11·1	24·1	8·1
11.	6·7	28·0	9·4	49.	11·1	20·5	7·4
12, 13.	6·1	21·9 (×2)	7·7 (×2)	Interval.			
14.	6·1	20·1	7·6	82.	10·0	18·0	4·7
15.	7·2	31·1	10·3	83.	10·0	18·9	6·1
16.	10·0	30·0	10·2	84.	10·0	14·0	5·1
17.	8·3	29·4	8·5				

The two experiments have evidently gone on almost *pari passu* for months, the amount of hydrogen evolved gradually diminishing, but showing, at the same time, a certain dependence on the heat of the day.

Under the microscope the bubbles of gas are seen to form, not on the zinc, but among the copper crystals, and sometimes to make their appearance on the glass at some distance off.

From the position of platinum in the electro-chemical series we anticipated that the effect would be still more marked with that metal in a spongy state on the zinc. It was deposited from the tetrachloride, and, of course, thoroughly washed. There was only 0·6 metre of foil; but the following quantities of hydrogen were obtained :—

Day.	Mean temp.	Vol. in cub. centims.
1.	11·7° C.	143·6
2.	11·4	93·6
3, 4.	10·0	38·8 (×2)
5.	8·6	26·0
6.	10·8	21·0
7.	9·4	17·1
8.	7·7	12·3

The first action, therefore, was about five times as great as in the case of the copper; and it diminished more rapidly, doubtless through the zinc becoming more quickly protected by oxide.

Lest it might be contended that the free oxygen, usually present in distilled water, had been the means of starting this action, the experiment was repeated with water as free from oxygen as could be obtained by boiling. One metre of the same zinc-foil, covered with copper, was employed; and the result was nearly as before, 40 cub. centims. of gas being obtained the first day at the mean temperature of 9° C. This arrangement was taken advantage of to examine the effect of a high temperature. Without removing the delivery-tube, the contents of the flask were heated to near 100° C., when 123·5 cub. centims. of hydrogen were given off in ten minutes. The apparatus was allowed to cool, with the mouth of the tube under water, when the production of gas became small again; and after two days it was again heated nearly to the boiling-point, when it gave off 93·4 cub. centims. in ten minutes; after another period of two days it gave 64·1 cub. centims. and after three days more 132·1 cub. centims. in the first thirty minutes, 108·4 in the second thirty minutes, 94·3 in the third, and 89·9 in the fourth.

Iron and lead, under similar circumstances, also decomposed pure water; and the action of magnesium was greatly increased by conjunction with copper. The effect of the more negative metal was the same as would have been produced by an increase of heat.

In a practical point of view this experiment may serve as a ready means of preparing pure hydrogen; in a theoretical point of view, its interest seems to lie in the fact that the dissociation of a binary compound by means of two metals may take place at infinitesimally short distances, when it would not take place where the layer of liquid is enough to offer resistance to the current—and also in the correlation between this force and heat*.

P.S. March 14.—At the suggestion of Prof. Stokes, we tried to ascertain if the well-known influence of points had much to do with the separation of this hydrogen gas. Two thin plates of copper were taken, the one smooth, the other rough with electrolytically-deposited copper; these were separated from thin plates of zinc merely by pieces of muslin; and the metals were folded over at each end and hammered together. Each couple was placed in water; and for some days very minute bubbles of gas formed, but only at the junction of the metals, and about equally in each case.

As might be expected, this zinc in conjunction with copper is capable of decomposing other liquids than water. Chloroform yields readily to its power; and iodide of ethyl, which Prof. Frankland decomposed by zinc only at a great heat, is split up rapidly at the ordinary temperature.

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* Since the above was written we have accidentally heard that Dr. W. Russell has been working in the same direction.

IX. *Intelligence and Miscellaneous Articles.*

ON THE INFLUENCE OF PRESSURE ON THE LINES OF THE SPECTRUM*. BY M. L. CAILLETET.

THE experiments which I have the honour of making known to the Academy are founded on the spectroscopic examination of the induction spark produced between two platinum wires sealed to the upper part of a tube of thick glass, in which a gas can be compressed to an exactly determined pressure.

The pressure is furnished by the apparatus described by me on the occasion of my researches on the compressibility of gases†. The glass tube which contains the platinum wires is joined to a reservoir in shape like a thermometer ; it thus contains a relatively considerable quantity of gas, which, when driven back by the mercury into which the open part of the reservoir dips, will occupy a height of several centimetres around the platinum wires. The apparatus containing the gas under experiment is enclosed in the laboratory tube of steel, and the upper portion of the glass tube is alone visible. To avoid accidents from its breaking, this tube is covered by a sheath of very transparent crystal. In case of explosion, as I have often proved, the projected fragments cannot break this envelope, which shelters the observer from all danger.

When, between the platinum wires, 2 or 3 millims. apart, the spark produced by a Ruhmkorff's coil excited by three Bunsen's elements is caused to pass, the light is very feeble and presents in the spectroscope pretty sharp lines standing out upon the scarcely illuminated background ; if then the pressure be given slowly and gradually, the lines soon become brighter and brighter, then widen, become less sharp, and at last dissolve in the field of the spectrum, which has become brilliant and vividly coloured. At that moment, if the pressure be still augmented, the electric light suddenly ceases.

I made many attempts to remedy this inconvenience ; but my efforts were not successful. I sought especially to avoid the deposit of a very small quantity of alkaline water which constantly condenses on the sides of the glass tube ; for this purpose I lined the interior of the tube with a varnish of gum lac, and inserted a piece of caustic potash. These various expedients did not sensibly retard the moment of the cessation of the passage of the spark. I augmented the intensity of the electric current employed ; the spark furnished by a coil of 3 decims. length excited by 8 large Bunsen elements had not sufficient energy to clear the space of less than $\frac{1}{2}$ millim. which separates the platinum wires.

A stronger electric current, by the sudden heating it produces, inevitably determines the breaking of the glass tube. It is at about 40 to 50 atmospheres that the sparks cease to pass ; the sides of the tube then become feebly luminous in the dark.

* I had recently, at Rome, the opportunity of seeing the Rev. Father Secchi ; and I am anxious to thank him for the encouragement and excellent advice he kindly gave me on the occasion of the experiments which constitute the subject of this Note.

† *Comptes Rendus*, vol. lxx. p. 1131.

The gases I have examined are hydrogen, atmospheric air, and nitrogen, previously dried by passing over caustic potash or concentrated sulphuric acid. The red line (α) of hydrogen assumes great splendour as the pressure increases; and when the tension of the gas is near 40 atmospheres, the red region of the spectrum is so luminous that the line α hardly stands out upon the brilliant background. At this moment the line γ is completely dissolved in the most refrangible portion of the spectrum. The lines of the other gases which I have examined produce the same effects; and the least sharp among them disappeared when the pressure became such that the luminous current ceased. In these experiments it often happens that the glass is attacked; then the sodium-line appears very brilliant. When the points of the wires are dipped in a salt of sodium, lithium, thallium, or one of the metals easy to recognize, the lines characteristic of these metals assume a splendour which goes on increasing with the pressure; and when the gaseous lines are almost effaced, the metallic lines, although much less sharp, are still conspicuous on the spectrum, which has become sensibly continuous. Several experiments have left me the impression that, if it were possible to observe under stronger pressures, continuous metallic spectra would probably be obtained. I have measured the proportion in which the luminous intensity of the spark increases under pressure. For this purpose I employ two induction-coils of the same dimensions, giving sparks of the same intensity. I then compress the gas contained in one of the luminous tubes; and I can compare, by one of the known means of photometry, the primitive spark with that increased by pressure. I have thus been enabled to prove that, by varying from 1 to 40 atmospheres the tension of the gas in which the spark is produced, the latter becomes at least 200 times as bright. In fact a spark which at the atmospheric pressure is scarcely visible, will under pressure light up a spacious laboratory.

From the facts here stated we may, I think, conclude :—

(1) That the spark which readily traverses the rarefied gas in a Geissler's tube or the electric egg, encounters considerable resistance when produced in compressed gas; it is likewise probable that the heating of the sides of the tube facilitates the flow of the electricity, as is shown by M. Regnault's experiments.

(2) That the brightness of the spark obtained under the ordinary pressure becomes at least 200 times as great when the tension of the gas is increased to the point at which the luminous current ceases. This confirms the beautiful experiments of Frankland on the combustion of hydrogen under pressure.

(3) That the luminous intensity of the gaseous lines increases with the pressure, and that at about 40 atmospheres, when the temperature must be very high in the vicinity of the wires, those lines almost entirely disappear in the field of the spectrum, which has become very luminous and sensibly continuous.—*Comptes Rendus de l'Acad. des Sciences*, May 13, 1872.

FURTHER RESEARCHES ON THE REFLECTION OF HEAT.
BY M. P. DESAINS.

In the sitting of the 22nd of April I had the honour of calling the

attention of the Academy to some effects of calorific reflection which are connected with the phenomena of anomalous dispersion ; and, on this subject, I was led to say that, on resuming the study of the reflection of polarized heat, I ascertained that when the rays are polarized parallel to the plane of incidence the intensity of the reflection is always given by Fresnel's formula $v^2 = \dfrac{\sin^2(i-r)}{\sin^2(i+r)}$, as well when the rays are partially transmissible through the mirrors as when the portion which escapes reflection is completely absorbed by the mirrors. I said also that in the case of the rays being polarized perpendicularly to the plane of incidence a very little modification of Fresnel's hypotheses or equations is sufficient for arriving at a formula very well representing the phenomena. In fact, if we admit that at the most superficial part of an opaque medium transmission commences as Fresnel conceives it to do in transparent media, but under the condition that the *vis viva* of the incident ray does not all reappear in the reflected and in the refracted ray properly so called, the equation of the *vires vivæ* may be written in the ordinary notation

$$(1-v^2) = \frac{\sin i \cos r}{\sin r \cos i} u^2 (1-\delta), \quad \ldots \ldots \ldots \ldots \ldots (1)$$

δ being a quantity of which the sign is at first indeterminate. If to this equation be added that which expresses the continuity of the motion in a direction parallel with the surface of the mirror a, viz.

$$(1+v)\cos i = u \cos r, \quad \ldots \ldots \ldots \ldots \ldots \ldots (2)$$

we hence readily deduce that the coefficient of vibration v in the reflected ray is given by the formula

$$\frac{\tan i - r}{\tan i + r} = -v\left(1 - \frac{\delta \sin i \cos i}{\sin i \cos i + \sin r \cos r}\right)$$
$$+ \frac{\delta \sin i \cos i}{\sin i \cos i + \sin r \cos r}. \quad \ldots \ldots \ldots \ldots \ldots \ldots \ldots (3)$$

So far δ is still indeterminate ; but I have ascertained empirically that all the determinations I have made are well represented by putting $\delta = K \tan^2(i-r)$, and giving to K the following negative values for the extreme red rays : $K = -0.19$ for platinum, and -0.22 for speculum-metal ; for glass and the same rays, $K = 0$. For glass and the extreme dark rays, $K = -0.8$; for speculum-metal and the same rays, $K = -1.9$. Lastly, for the total solar heat, but transmitted through a sufficiently considerable thickness of glass and Iceland spar, $K = -0.11$ in the case of steel ; and in that of silver, $K = -0.3$. For rock-salt, K is always $= 0$.

In each of these cases it is understood that the constant n preserves the value found for it in the corresponding case when operating with rays polarized parallel to the plane of incidence. All these assertions are verified by the following Tables :—

Intensity of Calorific Reflection under different Incidences.
Heat polarized parallel to the plane of incidence.

Angles of incidence.	Platinum (red rays, $n=8$). Observed intensities.	Calculated intensities.	Speculum-metal (red rays, $n=8.7$). Observed intensities.	Calculated intensities.
30	0·65	0·65	0·65	0·67
50	0·72	0·72	0·74	0·74
72·30	—	—	0·87	0·87
76	0·856	0·88	—	—

	Steel (total solar heat, $n=7.4$).		Silver (total solar heat, $n=20$).	
30	0·64	0·63	0·84	0·84
50	0·69	0·70	0·87	0·88
70	0·83	0·83	0·936	0·934
76	0·87	0·87	—	—
80	0·88	0·91	0·954	0·96

	Glass (extreme dark rays, $n=1.7$).		Speculum-metal (extreme dark rays, $n=26$).	
20	0·074	0·077	—	—
30	0·11	0·093	0·87	0·87
50	0·168	0·163	—	—
60	—	—	0·93	0·93
70	0·375	0·373	0·94	0·95

Heat polarized perpendicularly to the plane of incidence.

Angles of incidence.	Platinum ($n=8, k=-0.19$). Observed intensities.	Calculated intensities.	Speculum-metal ($n=8.7, k=-0.22$). Observed intensities.	Calculated intensities.
30	0·59	0·58	0·62	0·61
50	0·51	0·51	0·577	0·55
70	0·43	0·43	—	—
72·30	—	—	0·42	0·42
76	0·40	0·40	—	—
80	—	—	0·45	0·45

	Steel (total solar heat, $n=7.4, k=-0.11$).		Silver (total solar heat, $n=20, k=-0.3$).	
30	0·57	0·54	0·80	0·80
50	0·47	0·47	—	—
70	—	—	0·81	0·79
76	0·27	0·26	—	—
80	0·30	0·30	0·83	0·84

	Glass (extreme dark rays, $n=1.7, k=-0.8$).		Speculum-metal (extreme dark rays, $n=26, k=+1.9$).	
20	0·06	0·06	—	—
30	0·05	0·05	0·83	0·84
50	0·027	0·028	—	—
60	—	—	0·79	0·80
70	0·095	0·092	0·76	0·75

In relation to the experiments made with the total solar radiation,

it is right to remember that in a solar spectrum, especially when it is obtained with glass and Iceland spar, the heat is almost entirely confined to a space which does not occupy one sixteenth part of the total length of the spectrum. The value of n employed in this case is the mean of those corresponding to the different single rays of that narrow region.

I have thought it proper to notice the accord shown in the preceding Tables; but I mention it as a fact, without wishing to enter into any theoretic discussion. Equation (1), which stands here in the same form as in my former communication, might quite as well be written

$$(1-v^2) = \frac{\sin i \cos r}{\sin r \cos i} u^2 (1+\delta).$$

Equation (3) would then become

$$\frac{\tan (i-r)}{\tan (i+r)} = -v \left(1 + \frac{\delta \sin i \cos i}{\sin i \cos i + \sin r \cos r} \right)$$
$$- \frac{\delta \sin i \cos i}{\sin i \cos i + \sin r \cos r};$$

and the values of K would become positive, but without changing their absolute quantity. I add, in conclusion, that a clerical error crept into the final equation in my last communication; that error is rectified here.—*Comptes Rendus de l'Acad. des Sciences*, April 29, 1872.

ON ELECTRICAL PYROMETRY. BY LIEUT. ABNEY, R.E., F.R.A.S., F.C.S., ASSISTANT INSTRUCTOR IN TELEGRAPHY, S.M.E. CHATHAM[*].

During the last few months I have been observing spectroscopically gun-cotton flame, and have obtained results which it is thought will prove of importance in calculating for the safe storage &c. of that combustible. During my researches it was necessary, after heating gun-cotton previously to ignition to various temperatures, that I should ascertain the true temperatures of the resulting flames. These I endeavoured to arrive at by subjecting platinum wire to the different heats, and passing a voltaic current through it, and noting the electrical resistances of the wire cold and when heated. In order to find the degrees of heat, I made a series of careful experiments with the same wire for a large number of known temperatures. The resistances thus obtained I found did not agree accurately with the formula given by Dr. Siemens. The formula that agrees correctly with my results is

$$r = \alpha + \frac{\beta}{t} + \frac{\gamma}{t^2} + \delta t \dagger.$$

This formula was arrived at theoretically by a consideration of the expansion of the metal longitudinally and in diameter, together with a probable value of the retardation of the current due to heat.

Every precaution against error was adopted in the experiments. The wire had a preliminary heating to a high temperature, and the value of any thermo-electric current taken into account.

The results of the spectroscopic analysis of the flame I propose to communicate in a future Number.

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[*] Communicated by the Author.   [†] Practically $\frac{\gamma}{t^2}$ may be neglected.

X. *On the Nature of Electricity.* By M. E. EDLUND[*].

PART I.

IT was formerly the received opinion that heat consisted of a subtile and imponderable substance emitted by the source of heat and received by the body which was heated by it, the greater or less quantity of this substance determining the temperature of the body. According to an analogous theory, light also was composed of an imponderable substance of the same kind. To explain magnetic phenomena recourse was had to a new material, the "magnetic fluid;" and for electric phenomena a second fluid had to be admitted, which, like the magnetic fluid, must be composed of two distinct kinds. In regard to light and heat, it is now proved that these phenomena are oscillations, either of the minutest particles of matter or of the æther—that subtile and elastic material diffused through all nature, and even into every part of space unoccupied by any other matter. Since the discovery of diamagnetism, magnetic phenomena can no longer be accounted for by means of the magnetic fluids; while their electric origin can be demonstrated with the aid of Ampère's theory. The two electric fluids are therefore the only ones still considered necessary from a theoretical point of view. We shall, in this memoir, endeavour to show that electric phenomena, static

as well as dynamic, can be explained with the aid of a single fluid, which in all probability is no other than the æther*.

We assume the existence of a subtile, in the highest degree

* We take the liberty of borrowing, from the address delivered by Baron F. de Wrede, in 1847, on retiring from the Presidentship of the Royal Academy of Sciences, the following lines on the importance of the æther:— " One can no more admit that a substance filling infinite space, and exhibiting properties so peculiar and remarkable as those which we must necessarily attribute to the æther, has been destined by Providence solely to propagate light, than we can suppose that the air exists exclusively for the propagation of sound. The slightness of the density of the æther is proved by the total inappreciability of its resistance to the planets, which appear to move in it without impediment. The comets, on the contrary, the tenuity of which is singularly extreme, and which move with some of the greatest velocities in certain parts of their orbits, seem to experience a sensible resistance from the æther. If this fact be verified, the existence of the æther as matter endowed with inertia will be found established by a second method. On the other hand, the prodigious rapidity with which light is propagated shows us that the æthereal matter must possess extraordinary elasticity in comparison with its density. Of all the material substances within the limits of our experience, iron is the most elastic, and hydrogen (which has only about one fourteenth of the weight of atmospheric air) the lightest. Now a substance equal in density to hydrogen rarefied to about 1 millim. pressure, and as elastic as iron, would propagate sound (or any other vibratory motion) with a velocity of 8000 myriametres $(49,710\frac{1}{2}$ miles) in a second. Immense as this velocity is, it constitutes only about one fifth of that of light; and the modulus of elasticity of the æther, expressed in terms of the length, must consequently be about 25 times that of the hypothetic substance here taken for comparison. If we regard the æther as a gas, and imagine the possibility of a vacuum in it, the velocity with which the æther would rush into that vacuum would amount to 64,000 myriametres in a second; and estimating its density at the very lowest, its mechanical effects might, with that velocity, become singularly violent. It is therefore a thing in itself very probable that the æther plays one of the most important parts in almost all natural phenomena."

We permit ourselves also to quote the following from the end of Lamé's celebrated *Leçons sur la théorie mathématique de l'élasticité des corps solides* (Paris, 1852):—

" The existence of the æthereal fluid is incontestably demonstrated by the propagation of light in the planetary spaces, by the explanation so simple, so complete, of the phenomena of diffraction in the theory of undulations; and, as we have seen, the laws of double refraction prove with no less certainty that the æther exists in all transparent media. Ponderable matter is therefore not alone in the universe; its particles float, in a manner, in the midst of a fluid. If that fluid is not the sole cause of all the facts observable, it must at least modify them, propagate them, complicate their laws. It is, then, impossible to arrive at a rational and complete explanation of the phenomena of physical nature without interposing this agent, whose presence is inevitable. It cannot be doubted that that interposition, wisely guided, will discover the secret, or the true cause, of the effects which are attributed to caloric, to electricity, to magnetism, to universal attraction, to cohesion, to chemical attractions; for all these mysterious and incomprehensible beings are, in the main, merely hypotheses of coordination, doubtless useful in our present ignorance, but eventually to be dethroned by the progress of true science."

elastic substance diffused throughout the universe, not only in empty space, but also in the parts occupied by ponderable matter. We likewise assume that two molecules of æther, placed *at a distance* one from the other, repel each other along their line of junction and in the inverse ratio of the squares of the distances. The electric æther therefore most closely resembles an ordinary gas. With respect to the relations of the æther to all other matter, the only hypothesis we need to make is, that, in the bodies called good conductors of electricity, the æther contained in them (or at least a portion of it) moves with facility from one point to another. We suppose further that, in imitation of what takes place in an ordinary gas, the molecules of the electric æther move readily—that is, can be displaced by the least effort. In a nonconductor of electricity this mobility is arrested, and depends on that of the molecules of the material substance which contains the æther. If the nonconductor is a gas or a perfectly fluid liquid, the æther particles conserve their mobility, transporting themselves then with the gas or liquid particles. From this mobility it necessarily follows that the hydrostatic pressure must be equal in all directions, as in liquids and ordinary gases. To the æther, therefore, can be applied the principle of Archimedes, that a body introduced into a fluid loses a quantity of weight equal to the weight of the fluid displaced—although of course we have here to do, not with gravity, but repulsion between the æther molecules. Much light has been thrown upon the application of this principle to the question before us by some of Plücker's well-known diamagnetic experiments. He found that a magnetic body with a magnetic force inferior to that of the liquid in which it was suspended was repelled by the poles of the magnet, and that a diamagnetic body suspended in a magnetic liquid was more strongly repelled by the same poles than when it was in a less-magnetic liquid or gas[*]. An æther molecule is at rest from the moment in which it is equally repelled on all sides. A material body cannot be moved by electrical action, if the æther contained in it is equally repelled on all sides. If the repulsion is less on one side than on the other, the body must, if free, move in the direction determined by the resultant of the repulsive forces. To determine the motion produced in a body B in consequence of another body, A, being in its vicinity, we may, without restricting in any degree the solution of the problem, regard A as fixed and motionless, and B alone as free. It will then be necessary to take into consideration :—

1. The action exerted directly between the æther of A and that of B.

[*] Pogg. *Ann.* vol. lxxvii. p. 578.

2. The action on the æther of B of all the surrounding me-
dium, with the exception of the æther contained in A.

3. The action of the æther of A upon the æther which, if B
were removed, would occupy the space now occupied by B.

4. The action of all the surrounding medium, except the space
occupied by A, on the æther which, if B were away, would
be found in the space last occupied by B.

Evidently we have thus taken into account all the active causes.
The first two cases have reference to the effect of all the sur-
rounding mass of æther upon the æther of B; the last two, on
the contrary, express the same effect on the æther which would
be found in the place now occupied by B, if B were taken away.
Now, by taking the algebraic sum of the first two cases, and
subtracting from it the sum of the last two, we obtain, in accord-
ance with Archimedes's principle, the expression of the motion
produced in B. This will be made evident by the applications
we shall immediately make.

2. *Electrostatic attractions and repulsions.*—We assume that a
body which is said to be charged with positive electricity con-
tains more æther than when it is in the normal state, and that
the quantity of æther in an electro-negative body is less than
when its electric state is normal. The contrary might, perhaps,
be assumed; but several electrical phenomena seem to indicate
that the above-mentioned hypothesis is the true one.

Naming a the quantity of æther contained in the bodies A
and B in their normal state, we will first consider the case in
which both are positive—A having the excess b, and B the ex-
cess b_1. If the distance between the two bodies be r, and great
enough in proportion to the volumes of the bodies, the direct
repulsion between them can be expressed by

$$- \frac{(a+b)(a+b_1)}{r^2}.$$

The action on B of all the surrounding medium, except the
space occupied by A, has evidently a resultant equal to the re-
pulsion which takes place between B and the æther of the space
occupied by A, and in a direction opposed to that repulsion.
This is clear; for if A were removed, the resultant of the repul-
sion effected on B by all the surrounding medium would be
$= 0$; consequently the action on B of all the surrounding me-
dium except the space occupied by A is the same as if B were
attracted by that space. We thus get, as the expression of the
action implied in this second case

$$+ \frac{a(a+b_1)}{r^2},$$

the sign $+$ denoting that the action consists of an attraction along the line of junction.

The action in case 3 will evidently be $-\dfrac{(a+b)a}{r^2}$; and that of case 4, $+\dfrac{a^2}{r^2}$.

Subtracting the algebraic sum of the last two expressions from that of the first two, we obtain the result

$$-\frac{bb_1}{r^2}.$$

The repulsion between two electropositive bodies is therefore proportional to the product of the two excesses divided by the square of the distance.

We will now consider the case of the two bodies being electro-negative—that is, possessing less than the normal quantity of æther.

$$\text{The direct action between the two bodies will be} \left.\right\} = -\frac{(a-b)(a-b_1)}{r^2}.$$

The action foreseen in case 2 . . $= +\dfrac{a(a-b_1)}{r^2}$;

,, ,, ,, 3 . . $= -\dfrac{(a-b)a}{r^2}$;

,, ,, ,, 4 . . $= +\dfrac{a^2}{r^2}$.

Subtracting from the sum of the first two expressions the sum of the last two, we obtain for the action in this case the expression $-\dfrac{bb_1}{r^2}$.

Therefore the bodies repel each other in proportion to the product of the two deficiencies, and in the inverse ratio of the squares of the distances.

Let us suppose, in the last place, that A is electropositive and B electronegative; further, let b be the excess of A, and b_1 the deficiency of B. The four cases will give:—

$$1. \quad . \quad . \quad . \quad = -\frac{(a+b)(a-b_1)}{r^2};$$

$$2. \quad . \quad . \quad . \quad = +\frac{a(a-b_1)}{r^2};$$

$$3. \quad . \quad . \quad . \quad = -\frac{(a+b)a}{r^2};$$

$$4. \quad . \quad . \quad . \quad = +\frac{a^2}{r^2}.$$

Hence we shall obtain, by the same process as before, the following expression for the attraction between the two bodies :—

$$+ \frac{bb_1}{r^2}.$$

The attraction here, therefore, follows the known law.

Let us now suppose that a body A, with excess of æther, acts upon another body, B, originally in the normal condition, and a good conductor of the æther. A having an excess of æther, the repulsion upon all the æther molecules of B will be stronger on the side opposite to A than on any other. The necessary result of this will be, that the æther will collect on the side of B which is not turned towards A, leaving a deficit on the side facing A.

If, on the contrary, A has a deficiency of æther, any molecule whatever of the æther of B will necessarily be more strongly repelled by the surrounding medium in the direction towards A than in any other; here, then, an excess of æther will be formed, accompanied by a deficit on the opposite side.

It is evident that, in both these cases of induction, attraction must be produced between the two bodies; for the distance between the excess of one and the deficit of the other is always less than the distance between the two deficits or the two excesses.

It is easy to demonstrate that the excess or deficit of æther must place itself at the surface of the body. Let there be a body, A, having a certain quantity of æther $a + b$, of which b is the excess. It is evident that the æther of the surrounding space and the quantity a in A must balance each other, and therefore can exert no action upon a molecule of the excess b. With regard to the distribution of the excess, it is just as if the whole of the surrounding quantity of æther and the quantity a in A did not exist; the excess, then, must behave as if it alone existed; and in that case it would place itself at the surface, as Poisson has demonstrated.

That the deficit must equally place itself at the surface can be demonstrated in the following manner. Let us at first suppose that the body A contains the same quantity of æther as in the normal condition. Then any molecule of the æther of A is in equilibrium, seeing that all the repulsions annul one another, or, in other terms, have a resultant $= 0$. It follows that the resultant of the repulsions of all the molecules of the surrounding medium must be equal to the resultant of the repulsions of the æther molecules within the body, and act in a direction opposed to the latter. But we know that the æther molecules of the body tend, in consequence of their mutual repulsion, to place themselves at its surface. The resultant of the repulsion of all

the external molecules of æther must therefore tend to repel the æther within the body from the surface to the central parts. Let us now suppose a body presenting a deficit of æther, having less than the quantity required for equilibrium; the resultant of the repulsion of the external molecules will necessarily have the preponderance, and the æther molecules of the body will be driven from the surface to the interior. As, then, the body contains a less quantity of æther now than when it is in the neutral state, the result must be a deficit at the surface.

The condensation of the æther when a Leyden jar or a Franklin's tray is charged may be explained in an analogous manner: the current of the electric discharge is simply the passage of the æther out of one body into another*.

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* As we know, Franklin attempted to explain the electric phenomena known in his time by admitting only one electric fluid. He could not, however, account for the repulsion between two electronegative bodies without attributing to ponderable matter properties which it does not possess. For this cause the opinion of Franklin and the " unitarians " on the nature of electricity had to give place to that of the dualists, who established the hypothesis of the two fluids, admitted to this day. Some attempts have been made recently with a view to explain electric phenomena as produced by the æther or a single fluid. Without entering into a detailed account of these more or less happy endeavours, we think it necessary to observe that, relatively to the properties or the motions of the æther, they are founded on premises the justness of which may with reason be questioned,—and, further, that the theories in which they have ended are far from having the seal of simplicity, which assuredly they would have had, if they had been the real interpretation of the facts. The theory of light presupposes that the æther in a ponderable body varies in density with the body, and that its density remains the same as long as the body undergoes no modification. It must, consequently, be admitted that different sorts of ponderable matter exert different degrees of attractive force upon the æther molecules. A material body condenses within itself æther from the surrounding mass of æther until the resultant of the effects produced upon an external æther molecule by the proper molecules of the body and by the excess of æther included within the body becomes =0. With a body thus saturated the repulsion between its excess and an external æther molecule is equal to the attraction between the same molecule and the material molecules of the body. If, then, we are forced to admit, for the explanation of luminous phenomena, that, in virtue of the attraction exerted upon it by matter, the æther presents degrees of density varying with the bodies, it does not follow that on this account the bodies must exhibit certain electric properties. If, on the contrary, we increase or diminish in one way or other the quantity of æther which the body contains in its normal state, electrical phenomena begin to show themselves. It nevertheless is not an immediate consequence of this, that electrified bodies must give evidence of other optical properties than in their natural state. The velocity of propagation of light, and consequently also the wave-lengths, do not depend exclusively on the density of the æther, but on the ratio between its elasticity and its density. If, therefore, the elasticity of the æther be increased or diminished proportionally with its density, no modification can happen relative to the velocity of propagation of light, refraction, &c. The fact

3. *Electrodynamic phenomena.*—The galvanic current consists, in our opinion, in this :—that the electric æther moves from one point to another in the circuit, the intensity of the current being determined by the product of the density of the æther in motion and its velocity ; or, in other terms, it is proportional to the quantity of æther which passes through the circuit in the unit of time. The quantity of æther in the closed circuit is the same when the current exists as when there is no current. The electromotive forces from which the current derives its origin cannot create æther ; their action is restricted to transforming into translatory motion the oscillatory motion which already exists under the form of heat. From this it follows that heat should disappear at the point in the circuit where the electromotive force is acting—which is proved, moreover, by Peltier's phenomena. In this way the origin of the galvanic current becomes singularly simple : the electromotive forces create nothing new ; they merely, like ordinary machines, transform one sort of motion into another.

The numerous experiments which have been made for the purpose of ascertaining the velocity of electricity in wires have not given concordant results ; the reasons for this are easily comprehended. Wheatstone and Faraday have shown the important part played, in regard to this, by the conducting wire, in consequence of which a subsequent point of the wire cannot, at the commencement of the current, receive electricity until the preceding parts of the same wire have been saturated. The velocity of electricity in a conducting wire surrounded by an insulating layer and deposited in the sea should therefore appear relatively a minimum ; for the wire enclosed in the insulating layer and the water circulating around it constitute the armatures of a condensing apparatus capable of condensing a great quantity of electricity. The power of condensation of a wire insulated in the air is inferior to that possessed by a marine cable ; but it depends, in a great degree, on external circumstances, such as the humidity of the air, the mode of suspension, &c. Experiment also shows that submerged wires furnish the least amount of velocity. It has consequently not been possible to give determinate numbers for the absolute velocity of propagation of electricity ; but all the experiments agree in this, that it is singularly great, and that it is independent of the intensity of the current. Experiments with a single wire, and in identical circumstances, must give sure results.

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that certain experiments (Pogg. *Ann.* vol. cxxiv. p. 507) have shown the same optical properties in electrified bodies as when they are in their neutral condition, invalidates therefore not at all the proposition that electrical phenomena are produced by the æther.

In the sequel we shall make use of a proposition which has not, to our knowledge, been yet established as a principle in the explanation of natural phenomena, but which appears to us none the less axiomatic. This principle is, that every thing which takes place or is effected in external nature requires a certain time. The time may be as short as we will, but is never $=0$. "Time and space are conditions indispensable to the existence of natural phenomena" is an *à priori* truth ascertained by experiment in proportion as scientific methods for the measurement of time and space have been improved. For example, it was formerly believed that light and electricity propagated themselves instantaneously; but better methods of observation have shown that this is by no means the case. We may be perfectly assured that a galvanic current does not at once arrive at its whole force, and does not disappear without occupying a certain time in doing so, independently of the extra currents which retard these two phenomena. We must reject as absurd the proposition that the action exerted by a material body upon another at a certain distance, or the repulsion of one molecule by another at a distance, does not require time to propagate itself from the one to the other. However short the time may be, it always exists, even if it escapes our observation. When reciprocal action between two material bodies or two molecules of æther commences, it does not attain in a mathematical instant the full value determined by the reciprocal distance. It must increase from zero to that final value, and must have time for this. In like manner an action cannot vanish or change its amount without time being necessary for these effects. The above-formulated proposition, " Every thing which takes place or is effected in external nature requires a certain time," may, with regard to its importance, be compared with that which may be said to constitute the foundation of the mechanical theory of heat, and which is expressed in the words " nothing springs from nothing " (*ex nihilo nihil fit*). The proposition we have established must find its application particularly in the domain of electricity, seeing that the great velocity of propagation of this phenomenon calls forth rapid modifications in the reciprocal action exerted by the æther molecules upon each other. According to the determinations given by Fizeau and Gounelle, electricity is propagated in a copper wire with a velocity of 180 metres in the millionth part of a second; so that in this brief fraction of time two molecules of æther can diminish or augment their reciprocal distance by 360 metres, and their action upon one another be modified in consequence. The question now is whether this modification in the reciprocal action can be accomplished with a velocity corresponding to the rapid variation of the distance. Electrodynamic phenomena furnish the answer.

Let two molecules of æther, m and m', be at a distance r from one another. If both are at rest, their reciprocal repulsion is $\frac{mm'}{r^2}$. On the contrary, the case of m approaching or receding with a constant velocity gives rise to other ratios. If m be at first at x (fig. 1), at the distance $r + \Delta r$ from m', and then during the time Δt approach m' by the distance Δr, the reciprocal repulsion increases from $\frac{mm'}{(r + \Delta r)^2}$ to $\frac{mm'}{r^2}$; but if the velocity of approach be sufficient, the repulsion has not time to acquire that augmentation, and therefore at y is inferior to that

Fig. 1.

which corresponds to the distance r. This diminution, all circumstances being equal, is a function of the constant velocity h. The repulsion at y may therefore be expressed by $\frac{mm'}{r^2} f(h)$, where the value of $f(h)$ is less than 1. If, on the contrary, m recedes from m' with the same constant velocity h, passing in the time Δt through the distance $y - x'$ (fig. 2) $= \Delta r$, the repulsion at the moment when m arrives at y must be greater than that which corresponds to the distance r, provided the repulsion cannot be diminished with the velocity of the

Fig. 2.

increase of the distance. Therefore the repulsion may in this case be expressed by $\frac{mm'}{r^2} F(h)$, where $F(h)$ is greater than 1. If in the first case, in which the distance between the molecules is diminished, the velocity be considered negative, it must be positive in the second. Concerning the functions $f(h)$ and $F(h)$ we know nothing beforehand, except that the former must be less, and the latter greater than 1, and that both approach 1 as h diminishes. But as the causes which retard or accelerate the development of the repulsion at the time of the approach, must have the same effect upon its disappearance when the molecules recede from each other, it is probable that the two functions have the same form, or that the development of the repulsion follows the same law as its disappearance—and that both can be expressed by the same function of the velocity, if we take care to put the latter negative in one case and positive in the other. We have thus, for the repulsion between two æther molecules, the expression $\frac{mm'}{r^2} F(-h)$ if the molecules approach each other with a constant velocity h, and the expression $\frac{mm'}{r^2} F(+h)$ if the
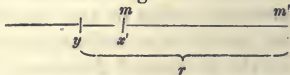
distance between them increases, the function F being such that it becomes $=1$ for $h=0$, is <1 for a negative value, and >1 for a positive value of h. These expressions may conveniently be written in the form

$$\frac{mm'}{r^2}\left(1+\phi(-h)\right) \text{ and } \frac{mm'}{r^2}\left(1+\phi(+h)\right),$$

the function $\phi(h)$ being such that it becomes $=0$ when $h=0$, has a negative value when h is negative, and a positive value when h is positive.

What has just been said applies exclusively to the case in which the velocity is constant. We will now suppose that m approaches m', and makes the same way Δr in the same time Δt as before, but with *diminishing* velocity, so that the velocity is greater when m is nearer x (fig. 1) than when it has arrived at y. Although m makes the same way during the same space of time, and consequently $\frac{\Delta r}{\Delta t}$ has the same value in this as in the former case, the repulsion at the point y can no longer be the same. The molecule m has moved more rapidly in the vicinity of x than when nearer to y, and has therefore remained longer where the repelling force is stronger than where it is weaker. The result must evidently be that the repulsion at y will be stronger than if the velocity had been constant. The repulsion, then, depends not only on $\frac{\Delta r}{\Delta t}$, but also on $\frac{\Delta^2 r}{\Delta t^2}$. If we now pass to the limit, we thus find that the repulsion does not depend merely on the velocity, but again on the variation of the velocity, $\frac{dh}{dt}$, the latter dependence *augmenting, in the present case,* the quantity of the repulsion-force.

If the molecule m increase its distance from m' while its velocity *augments,* but in such a way that the determined path Δr is traversed in the fixed time Δt, the repulsion in this case, as in the preceding, will be greater than if the velocity were constant. Here also the molecule will remain longer at the points where the repulsion-force is greater, than at those where it is less. It is therefore necessary to add to the expression representing the amount of the repulsion under constant velocity a term dependent on the variation of the velocity.

The electric molecule moves in its course with a constant velocity; as was said above, variations in the intensity of the current exert no influence in this respect. If, therefore, a molecule approaches or recedes from another which is on the straight line in which the movement takes place, there can be no variation in

the relative velocity.   The circumstances, on the contrary, are different if one of the molecules is on one side of the direction of the other.   Suppose two molecules, m and m', the first of which is in motion on the line $a\,b$ (fig. 3), and the other, m', at rest.   The distance r between the molecules is then equal to $\sqrt{x^2 + p^2}$; and their relative velocity (that is, the velocity on the line of junction)

$$\frac{dr}{dt} = \frac{x}{r}\frac{dx}{dt}.$$

Fig. 3.

Therefore the relative velocity diminishes as m approaches the point o, where it is $=0$.   When, on the contrary, the distance between the molecules increases, their relative velocity increases simultaneously.   The variations of the relative velocity are obtained by differentiating the last expression, which gives

$$\frac{d^2r}{dt^2} = \frac{dx^2}{r\,dt^2} - \frac{x^2}{r^3}\frac{dx^2}{dt^2};$$

or if we introduce the cosine of the angle in the place of $\dfrac{x}{r}$, and h in place of $\dfrac{dx}{dt}$, we obtain

$$\frac{d^2r}{dt^2} = \frac{h^2}{r}\,(1 - \cos^2\theta).$$

The variation of the relative velocity, therefore, is proportional to the square of the velocity of the molecule in the circuit; it presents its maximum at the point o (fig. 3), and diminishes as the molecule moves away from it.   By corresponding substitutions we obtain for the expression of the relative velocity

$$\frac{dr}{dt^2} = \cos\theta h.$$

If the molecule m moves with a constant velocity on the line $a\,b$ (fig. 3), in which case the relative velocity varies in relation to the fixed molecule m', the repulsion between the two molecules for a determined distance r is, according to what precedes, greater than if the relative velocity were constant.   This is so, whether m recedes from, or whether it approaches the point o. To the expression denoting the repulsion between the two molecules when their relative velocity is constant we must therefore add a term constituting a function of the variation of the velocity. We will designate this function by $\psi\left(\dfrac{h^2}{r}[1 - \cos^2\theta]\right)$.   We know beforehand, with respect to this function ψ, that it must
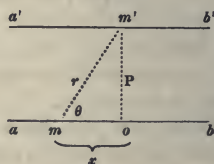
be $=0$ when $\cos\theta=1$, since in this case the molecule m moves in the line of junction between m and m', and consequently the relative velocity of the two molecules is constant. We know moreover that the value of the function ψ *is always positive,* whether the molecule m approaches or recedes from m'. It may, besides, be remarked that the value of the function may depend not only on the amount of the variation, $\dfrac{h^2}{r}(1-\cos^2\theta)$, but also on the distance r between the molecules, and consequently r may enter under the sign of the function at the same time that the same variable enters into the expression of the quantity of the variation.

The complete expression of the repulsion between two molecules of æther m and m', the latter of which is fixed, and the former, m, moves with a constant velocity h in a line forming the acute angle θ with their line of junction, will therefore be :—

When m approaches m',

$$-\frac{mm'}{r^2}\left[1+\phi(-h.\cos\theta)+\psi\left(\frac{h^2}{r}[1-\cos^2\theta]\right)\right]; \quad (1)$$

When m recedes from m',

$$-\frac{mm'}{r^2}\left[1+\phi(+h.\cos\theta)+\psi\left(\frac{h^2}{r}[1-\cos^2\theta]\right)\right]. \quad (2)$$

What has just been said we shall first apply to the case of two molecules m and m' moving with equal and constant velocity in the same direction in parallel lines (see fig. 3).

According to the principles established by W. Weber[*], we shall admit that the effect of the reciprocal action between two molecules is entirely communicated to the circuits in which they move. The motions only of the circuits can be observed in the reciprocal action of two currents; and the empirical formulæ founded on the observations relate to those motions. Now, in order to find the variation produced in the distance between two circuit-elements by the reciprocal action of the æther molecules, one of the elements may be regarded as fixed, and the other alone as free. We suppose, in the present case, that the element in which m' moves is free, and that which belongs to m is immoveable. If in the whole mass of æther the molecule m' were alone in motion, it could not be admitted, in the same manner as if it were at rest, that the repulsions exerted upon it by the whole of the surrounding æther annul each other; on the contrary, those repulsions might have a resultant S not $=0$. The repulsion exerted on the moving molecule m' by all the surrounding æther with the exception of m should therefore be ob-

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[*] *Abhandlungen über elektrodynamische Maasbestimmungen,* p. 309.

tained by deducting from S the repulsion which takes place between m and m'—or, what comes to the same thing, by adding to S the latter repulsion taken with the opposite sign. The question now is, what is the motion impressed on the circuit-element in which m' moves by the molecule m being put in motion?

In the same way as for electrostatic phenomena, we have to take into consideration the four following circumstances:— 1, the direct reciprocal action of the two molecules; 2, the difference between the action exerted upon m' by *the whole* of the surrounding æther when m is supposed at rest and the action exerted upon the same molecule m' by all the æther *with the exception of m*; 3, the action of m upon the space occupied by m'; and, 4, the action upon the same space of all the surrounding æther with the exception of m. The difference mentioned at no. 2 is evidently equal to the repulsion, taken with the opposite sign, between m, supposed immoveable, and the molecule m'; and the action indicated at no. 4 is identical with the repulsion, taken with the opposite sign, between the molecule m regarded as immoveable and the space in question. If we add the actions upon m', foreseen in the first two cases, and if we subtract the corresponding sum of the last two, we obtain, in accordance with Archimedes's principle, the action upon m' sought, or upon the circuit-element in which m' moves.

In order to understand more clearly the accuracy of the above process, let us state the problem thus:—To find the motion produced in the molecule m', or in the circuit-element in which m' is found, by the molecule m being put in motion. Now the motion sought depends evidently on the modification induced in the repulsion between m' and m by the circumstance that the latter has been put in motion. The expression of the motion of the circuit-element of m' is therefore obtained by subtracting from the repulsion between the molecules m' and m (the latter being regarded as in motion) the repulsion between the same molecules when m is considered to be at rest. The remainder thus obtained is in reality the sum of the first two cases above stated. The effects of repulsion to which the last two cases relate are obtained in an analogous manner. It is now easy to find the algebraic expression of the reciprocal action of two elements of a current. If the two molecules m and m' move in parallel lines in the same direction, as, for example, towards b and b', their reciprocal distance will undergo no modification, provided they move with the same velocity. Their direct action upon each other will thus be the same as if they were both at rest. We have, therefore, for the action belonging to case 1:—

$$-\frac{mm'}{r^2}.$$

As m' recedes from m if the latter is at rest, we have for case 2 :—

$$+ \frac{mm'}{r^2}\Big[1 + \phi(+h.\cos\theta) + \psi\Big(\frac{h^2}{r}[1 - \cos^2\theta]\Big)\Big].$$

For case 3, in which m approaches the space occupied by m', we obtain :—

$$- \frac{mm'}{r^2}\Big[1 + \phi(-h.\cos\theta) + \psi\Big(\frac{h^2}{r}[1 - \cos^2\theta]\Big)\Big].$$

In the last place, we have for No. 4 :—

$$+ \frac{mm'}{r^2}.$$

Subtracting now the sum of the last two expressions from the sum of the first two, we obtain the definitive result :—

$$+ \frac{mm'}{r^2}\Big[\phi(+h.\cos\theta) + \phi(-h.\cos\theta) + 2\psi\Big(\frac{h^2}{r}[1 - \cos^2\theta]\Big)\Big]. \quad (3)$$

This result is the theoretic expression of the reciprocal influence of two current-elements which move in the same direction in parallel lines.

By making, in formula (3), $\cos\theta = 0$ (that is to say, by supposing the line of junction between the two current-elements to form a right angle with the lines of direction of the currents), the function ϕ will become, as we have seen, $= 0$. We shall therefore have for this case :—

$$+ \frac{mm'}{r^2} . 2\psi\Big(\frac{h^2}{r}\Big). \quad . \quad . \quad . \quad . \quad . \quad (4)$$

Now, according to the preceding reasoning, the value of the function ψ is always positive. It hence follows that in this position the current-elements attract each other—a fact already demonstrated by experiment.

We will now compare the theoretic result with experiment, in order to determine the functions ϕ and ψ.

Ampère, as is known, has determined experimentally the mutual action of two current-elements; and W. Weber has proved by very accurate experiments the correctness of the results obtained by the French physicist. For the case in which the circuit-elements are parallel, r being their distance, and θ the angle made by one of them with their line of junction, Ampère's formula is

$$+ \frac{kii'}{r^2}\Big(1 - \frac{3}{2}\cos^2\theta\Big)ds\,ds', \quad . \quad . \quad . \quad . \quad (5)$$

in which i and i' denote the intensities of the two currents, ds and ds' the two circuit-elements, and k a constant. As long as this expression is positive, there is attraction between the circuit-

elements along their line of junction. If the two currents follow the same direction, and consequently have the same sign, the elements attract each other as long as the term $\frac{3}{2}\cos^2\theta < 1$. But if they go in opposite directions and therefore have contrary signs, repulsion takes place as far as that limit. If, now, μ and μ' denote the quantities of electricity in the unit of length of the two circuits, we shall have $\mu h = i$ and $\mu' h = i'$, h denoting the velocity of the current. Now μds and $\mu' ds'$ correspond to what in the theoretic formula were denoted by m and m'. Ampère's formula may therefore be written

$$+\frac{kmm'h^2}{r^2}\left(1-\frac{3}{2}\cos^2\theta\right). \quad \ldots \quad (6)$$

Making $\cos\theta = 0$, we obtain, by comparison with formula (4),

$$2\psi\left(\frac{h^2}{r}\right) = kh^2,$$

whence we derive, on replacing h^2 by $h^2(1-\cos^2\theta)$:—

$$2\psi\left(\frac{h^2}{r}[1-\cos^2\theta]\right) = kh^2(1-\cos^2\theta). \quad . \quad (7)$$

Making, in formula (3), $\cos\theta = 1$, the value of the function ψ becomes $= 0$. In this case the two current-elements are in one and the same line, by which their relative velocity becomes constant and $= 0$. Formula (3) thus becomes

$$+\frac{mm'}{r^2}[\phi(+h)+\phi(-h)]. \quad \ldots \quad (8)$$

Putting, in the same way, $\cos\theta = 1$ in the empiric formula (6), and comparing it with formula (8), we obtain

$$\phi(+h)+\phi(-h) = -\tfrac{1}{2}kh^2,$$

from which, substituting $h\cos\theta$ for h, we get

$$\phi(+h\,.\,\cos\theta)+\phi(-h\,.\,\cos\theta) = -\tfrac{1}{2}kh^2\,.\,\cos^2\theta. \quad . \quad (9)$$

Introducing now into the theoretic formula (3) the found values of the function ψ and the sum $\phi(+h\,.\,\cos\theta)+\phi(-h\,.\,\cos\theta)$, we obtain

$$+\frac{kmm'h^2}{r^2}\left(1-\frac{3}{2}\cos^2\theta\right),$$

which is identical with the formula derived directly from the observations.

Formula (9) determines the sum of the two functions ϕ. This sum is always negative. Of course we cannot immediately conclude from this the form of the function itself, since a term may have vanished in the addition. We know, from the preceding, that $\phi(-h)$ must always be negative, but, *per contra*, $\phi(+h)$ always

positive. This is only possible by one means alone, viz. that the function ϕ contains, besides the term into which the square of the relative velocity enters, a term into which an odd power of that velocity enters, and that the value of the latter term is greater than that of the former. We will now suppose the odd power to be the first—which is the only correct supposition, as will be seen when we consider two parallel currents in opposite directions. This gives us

$$\left.\begin{array}{l} \phi(-h.\cos\theta) = -ah.\cos\theta - \tfrac{1}{4}kh^2\cos^2\theta, \\ \phi(+h.\cos\theta) = +ah.\cos\theta - \tfrac{1}{4}kh^2\cos^2\theta, \end{array}\right\} \quad . \quad (10)$$

in which a is a constant. We have therefore obtained the same result as if we had imagined the function ϕ developed in a series according to ascending powers of the relative velocity, and retained only the first two terms of that series.

We now pass to the case in which the molecules m and m' move in opposite directions in parallel circuits. We suppose that the molecule m' moves towards the point a', while m advances towards the point b (fig. 3). It is evident that in this case the relative velocity of m and m' must be twice as great as if one of the molecules were to rest while the other moved with the same velocity h as before. $2h$, then, must be written in the place of h; and the same applies equally to the variation of the velocity. It makes no difference whether the molecules are approaching or receding from one another. Employing formulæ (1), (7), and (10), we obtain in this way, for the direct action between two molecules in motion (case 1):—

$$-\frac{mm'}{r^2}\left[1 - 2ah\cos\theta - \tfrac{1}{4}.4kh^2\cos^2\theta + \tfrac{1}{2}.4kh^2(1-\cos^2\theta)\right].$$

For the action to which no. 2 refers (viz. the repulsion, taken with the contrary sign, between the molecules m' and m, the former considered in motion, and the other in the state of repose) we obtain

$$+\frac{mm'}{r^2}\left[1 - ah\cos\theta - \tfrac{1}{4}kh^2\cos^2\theta + \tfrac{1}{2}kh^2(1-\cos^2\theta)\right].$$

We get, for the action foreseen in no. 3,

$$-\frac{mm'}{r^2}\left[1 - ah\cos\theta - \tfrac{1}{4}kh^2\cos^2\theta + \tfrac{1}{2}kh^2(1-\cos^2\theta)\right];$$

and for no. 4,

$$+\frac{mm'}{r^2}.$$

Subtracting now the sum of the last two numbers from that of the first two, we obtain as the expression of the action which two current-elements exert upon one another when they move in opposite directions in parallel circuits:—

$$-\frac{kmm'h^2}{r^2}\left[1-\frac{3}{2}\cos^2\theta\right], \quad . \quad . \quad . \quad (11)$$

which is found to be in full accordance with Ampère's empiric formula.

What has just been said refers to the supposition that the velocity h is the same in both circuits. It is easy, however, to prove that the above demonstration applies equally to the case in which the velocity is greater in one circuit than in the other. Let us suppose that the velocity in the circuit $a'b'$ (fig. 3) is h', while that in the circuit ab is equal to h, that $h' < h$, and that the motion is in the same direction in both circuits, viz. towards b and b'. It is evident that the relative velocity is not altered by the *absolute* velocity of both molecules being increased or diminished by an equal quantity. If each of the molecules m and m' receive a velocity h' in a direction opposite to the preceding, the molecule m' will come to rest, while m will continue to move in the same direction as before, but with the velocity $h-h'$. Consequently their relative velocity, according to the preceding considerations, will be $h-h'\cos\theta$. We obtain then, for the action no. 1,

$$-\frac{mm'}{r^2}\left[1-a(h-h')\cos\theta-\tfrac{1}{4}k(h-h')^2\cos^2\theta+\tfrac{1}{2}k(h-h')^2(1-\cos^2\theta)\right]$$

for the action no. 2,

$$+\frac{mm'}{r^2}\left[1+ah'\cos\theta-\tfrac{1}{4}kh'^2\cos^2\theta+\tfrac{1}{2}kh'^2(1-\cos^2\theta)\right].$$

No. 3 gives

$$-\frac{mm'}{r^2}\left[1-ah\cos\theta-\tfrac{1}{4}kh^2\cos^2\theta+\tfrac{1}{2}kh^2(1-\cos^2\theta)\right];$$

and, lastly, no. 4,

$$+\frac{mm'}{r^2}.$$

Subtracting the sum of the last two results from that of the first two gives:—

$$+\frac{kmm'}{r^2}\left[1-\frac{3}{2}\cos^2\theta\right]hh',$$

which, as is seen, agrees with experiment.

For the case in which parallel currents go with unequal velocities in opposite directions, in an analogous fashion the same result is obtained, but with the minus sign.

By means of the above considerations it is easy to deduce Ampère's general formula for the reciprocal action of two current-elements the positions of which are indeterminate. Now the empiric formulæ given by Ampère comprise the laws of all electrodynamic phenomena. We have therefore shown that these as well as electrostatic phenomena can be explained by the admission of a single fluid. Our demonstration rests upon two fundamental principles, viz.:—1, the principle of Archimedes, the applicability of which to phenomena of this kind appears theoretically incontestable, and has besides been proved by Plücker's experiments, cited at the commencement of this memoir; 2, the important proposition, which appears to us axiomatic, that every thing which takes place or is effected in external nature requires a certain time. Moreover we have had no need to attribute to the electric fluid any properties contrary to those which belong to the luminiferous æther.

Light, heat, and electricity thus become phenomena which take place in the same material; and by this these three principal groups of natural phenomena are brought into the closest relation with one another.

In descriptions of some phenomena connected with electricity we sometimes meet with the remark that they cannot be explained by means of a single electric fluid. Thus, for example, it has been maintained that the simultaneity of the sparks at the two extremities, in the well-known experiments of Wheatstone on the velocity of propagation of electricity, is a proof against the correctness of the admission of one fluid only. It appears to have been imagined that if we admit the existence of one electric material only, we must also admit that the *same* quantity of electricity forms the two sparks, so that the spark which is produced nearest to the negative armature of the battery cannot be produced until the electricity has had time to traverse both the conducting wires. We cannot subscribe to this view. The æther presents *less* density on the negative armature of the battery than on the conducting wire in contact with it. On the other hand, upon the positive armature the density of the æther is greater than on the wire which starts from this armature. At the time of the discharge of the battery a quantity of æther passes from the positive armature to the conducting wire in contact with it; but simultaneously another quantity of æther passes to the negative armature from the wire in contact with this latter. Consequently the two sparks appear simultaneously.

In like manner the difference between Lichtenberg's figures

when produced by positive and when by negative electricity has been regarded as a proof of the existence of two electric fluids. But, as we know that the difference disappears in a vacuum, it seems impossible to draw from it any certain conclusion for or against either opinion. It is doubtless the same with some other phenomena to which it has been thought we ought to attach some importance with respect to the matter in question.

On the other hand, several phenomena decidedly support the opinion that the substance which constitutes the basis of electric phenomena is simple and indivisible. We note, among others, the fact studied by Wiedemann and other physicists, that a liquid traversed by a galvanic current is mechanically impelled in the direction of the positive current. To account for this it is necessary to admit that the negative current does not possess this property at all, or at least that it possesses it only in a less degree—although for the explanation of several other phenomena it must be admitted that the positive and the negative currents behave identically in relation to matter. With the circumstance just stated is connected the known fact that the positive pole of a voltaic battery, and the positive knob in the formation of sparks, are principally, if not almost exclusively, corroded and destroyed. In the second part of this memoir we shall give the explanation of the fact that the negative pole is not left altogether intact, as well as of the phenomena investigated in so remarkable a manner by M. Quincke*. The idea which has been formed of the mode of propagation of the positive and negative current in opposite directions in a conductor is by no means simple, and is consequently any thing but natural. The explanation of these phenomena is infinitely more easy to understand by admitting the presence of one electric fluid only. The existence of the æther is as certain as that of the atmosphere which encompasses our globe. If, then, it is possible to prove that the phenomena of electricity can have their source in the æther, we may be perfectly well assured that there exists no special electric fluid; for if nature can produce certain phenomena by means of one agent only, she will not employ two.

<div style="text-align:center">[To be continued.]</div>

* Pogg. *Ann.* vol. cxiii. p. 513 (1861).

XI. *Reply to some Remarks* of the* Hon. J. W. Strutt *on Gaseous Pressure.* By Robert Moon, *M.A., Honorary Fellow of Queen's College, Cambridge*†.

IN criticising my doctrine that density alone does not determine pressure but velocity also requires to be taken into account, Mr. Strutt evidently thinks that he has placed me in a dilemma by asking, "What is here meant by velocity?" Is it "the *absolute* velocity which is intended," or does it mean "the velocity relatively to the containing vessel?"

To this I reply, certainly not the velocity relatively to the containing vessel, which, since "no account is taken of friction or viscosity," may, as Mr. Strutt has pointed out, have any arbitrary motion in a direction parallel to its axis without affecting the motion of the air within it.

But if I am asked whether the velocity to which I refer is inclusive or exclusive of the velocity which the aërial particles whose motion is being considered have in common with the contiguous particles of the earth's surface, I answer that it is a matter of indifference whether we do or do not include that common velocity.

If the expression which I have given for the pressure in terms of the velocity and density be referred to, it will be found to contain an arbitrary function, the form of which in any particular case of motion must be determined by a consideration of the relations existing between the pressure, density, and velocity under particular circumstances. The form of the function when the absolute velocity is dealt with will be different from that which occurs when the relative velocity is made use of; but the expression for the *pressure* in either case will be precisely the same.

I am unable to understand the import of the question, "Is it not obvious that the physical condition of a small mass of air is independent of any velocity animating all its parts?"

If a variable velocity affecting the different particles is here referred to, I answer in the negative; but if a uniform velocity common to all the particles be meant, I agree with my critic, at the same time regretting that he should not have taken more pains to ascertain my views before ascribing to me an opinion which would be simply preposterous.

Though Mr. Strutt is quite certain that my "analytical argument . . . . is fallacious," I am inclined to think that, upon reflection, he will change his mind upon the subject.

If we have three relations,

$$p = f_1(xt),$$
$$\rho = f_2(xt),$$
$$v = f_3(xt),$$

where the forms of f_1, f_2, f_3 are utterly unknown to us, the presumption is that

$$p = \text{funct. } (\rho, v).$$

This is the rule, to which there may possibly be exceptions; but those who rely on the exceptions must prove them to exist.

The only restriction upon the forms of f_1, f_2, f_3 is that they shall satisfy the equation

$$0 = \frac{d^2y}{dt^2} + \frac{1}{D} \cdot \frac{dp}{dx},$$

where x is the ordinate of the point of rest of a particle, y the ordinate at the time t, and D the density of equilibrium. Can my opponents prove, what Mr. Strutt in effect affirms that they maintain, viz. that the forms of f_1, f_2, f_3 are so moulded by this condition "that, when you eliminate x and t, v will disappear with them"?

That they have never attempted to do so, analytically, is certain; and that any attempt of the kind must have resulted in failure is equally so. The only experimental proof upon which my opponents can rest their conclusion is Mariotte's, or, as Mr. Strutt prefers to call it, Boyle's law. Let us see, therefore, the degree of support which this affords them.

Mr. Strutt has "never heard" that the law "was regarded otherwise than as a clear result of experiment."

Surely Mr. Strutt must be aware that Boyle's law only predicates the rate of pressure in an elastic fluid when in equilibrium, that every experiment upon which the law rests, down to those of Regnault, is a statical experiment and that neither Mariotte, Boyle, nor any one else has ever so much as attempted to prove experimentally the law which prevails when the fluid is in motion. That the law holds when the fluid is in motion is a pure assumption, and one which, with deference to Mr. Strutt, I must continue to characterize as *gigantic*.

That the law does not hold outside the limits within which it was originally proposed I have proved irrefragably by the adduction of instances* (which might have been multiplied indefinitely)

* See Phil. Mag. S. 4. vol. xxxvi. p. 27. In a paper of mine, an abstract of which appeared in the Proceedings of the Royal Society for 1862, the subject is treated in considerable detail. I regret that in the case first discussed the argument is so incorrectly stated in the abstract as scarcely to be intelligible. A further case of failure is pointed out in my June paper.

in which the law signally fails.   One of these is so simple, and to my mind so conclusive, that I shall repeat it here.

Suppose the tube A B to be filled with air which at the time t is destitute of velocity; the density of the air in A C being at
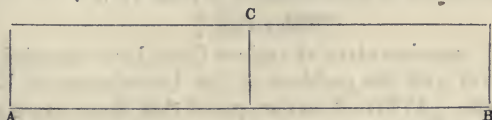
the same time uniform and equal to D, while that in B C is uniform and equal to 2D, the common boundary of the two portions of air being an imaginary plane through C perpendicular to the axis of the tube.   Under these circumstances, according to Mariotte's law, the pressure at the time t of the fluid in B C on the fluid in A C will be *double* the pressure of the fluid in A C on B C; *i. e.* the supposition of the truth of Mariotte's law in this instance contradicts the universally received principle that action and reaction are equal and opposite.

I can assure Mr. Strutt that I am not so profoundly ignorant of the history of the subject as ever to have supposed that Bernoulli, Lagrange, or Euler had considered it in the way in which I have done.   Beset with the analytical difficulties attendant upon the theory, believing (erroneously) that the law of pressure was beyond the scope of their analysis, and was only capable of being determined by experiment, those great writers adopted tentatively and not unnaturally the only experimental law of pressure which any one had ever heard or thought of, or indeed was ever likely to obtain—that, namely, of Mariotte or Boyle.   That they did so was fortunate; and benefit flowed from their processes, notwithstanding the error which they involved.   But persistence in that error can only work evil, by closing against mathematical investigation a field of inquiry admirably adapted for its exercise, and from its application to which the most brilliant results may be anticipated.

In conclusion I ask for some intelligible reason why the expressions for the pressure, velocity, and density given in my June paper are to be rejected.   They satisfy the equation of motion, the only test to which they can be subjected possessing the slightest claim to authority.   I must hope that eventually they will satisfy Mr. Strutt.

6 New Square, Lincoln's Inn,
    July 4, 1872.

XII. *Researches in Actino-Chemistry.*—Memoir First. *On the Distribution of Heat in the Spectrum.* By John William Draper, *M.D., LL.D., President of the Faculties of Science and of Medicine in the University of New York*.*

[With a Plate.]

MANY experimenters at various times have occupied themselves with the problem of the Distribution of Heat in the spectrum. At first it was supposed that there was a coincidence between the luminous and the calorific radiations, and that the maximum of intensity in both occurred at the same point—that is, in the yellow space. This view was abandoned on the publication of the well-known experiments of Sir W. Herschel, who showed that in certain cases the maximum is below the red. Subsequently, Melloni having discovered the singular heat-transparency of rock-salt, proved that when a prism of that substance is used the maximum in question is as far below the red as the red is below the yellow—but that if the light has passed through flint-glass the maximum approaches the red—if through crown glass, it passes into the red—if through water or alcohol, it enters the yellow.

In the case of the sun's spectrum the distribution of heat was more closely examined by Professor Müller, whose results confirmed in a general manner the view then held, that the invisible radiation below the red greatly exceeds that in the visible spectrum; and still more recently Dr. Tyndall, examining the spectrum of the electric light through rock-salt, showed that the curve indicating the distribution "in the region of the dark rays beneath the red shoots suddenly upwards in a steep and massive peak—a kind of Matterhorn of heat, which quite dwarfs by its magnitude the portion of the diagram representing the visible radiation." These investigations were made under unexceptionable circumstances: the beam of electric light had practically undergone no atmospheric absorption; and the optical refracting train was of rock-salt.

Sir J. Herschel had shown in 1840 that, when the sun's rays are dispersed by a flint-glass prism, the distribution of the heat toward the less-refrangible regions is not continuous, but there are three maximum points. These points, as shown by Dr. Tyndall, do not exist in the spectrum of electric light, the decline of which is perfectly continuous; they are therefore to be attributed to the absorptive disturbance which the sun's rays have undergone. Quite recently M. Lamansky has succeeded in identifying these interruptions by the aid of the thermomultiplier. In his memoir he states that, with the exception of

* Communicated by the Author.

Prismatic or Dispersion Spectrum.
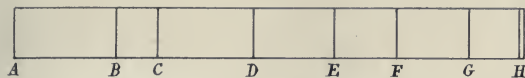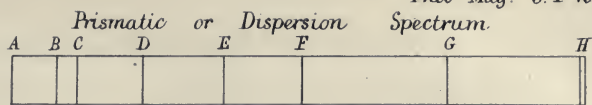
Diffraction Spectrum

Fig. 1.

Fig. 2.
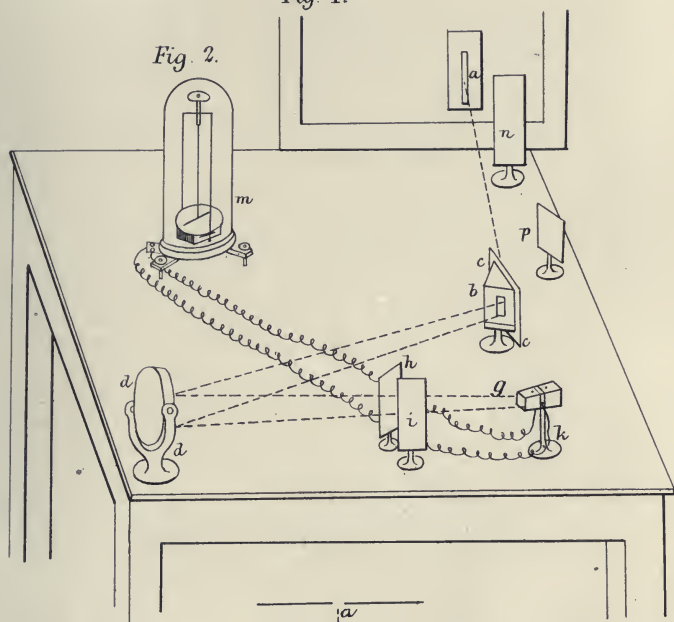
Fig. 3.
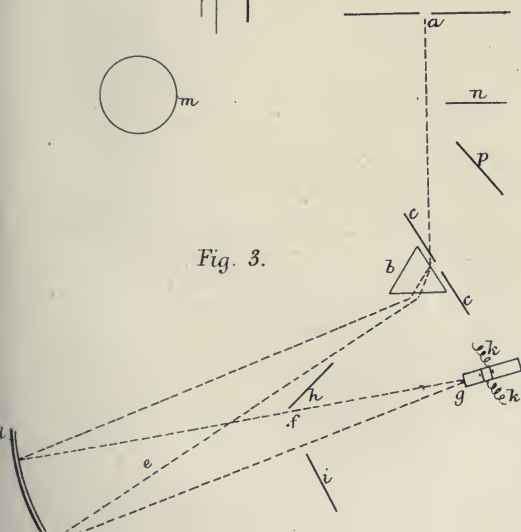
Fig. 4.
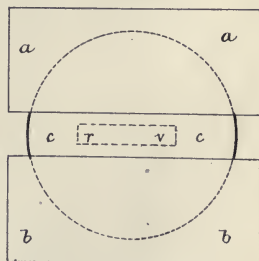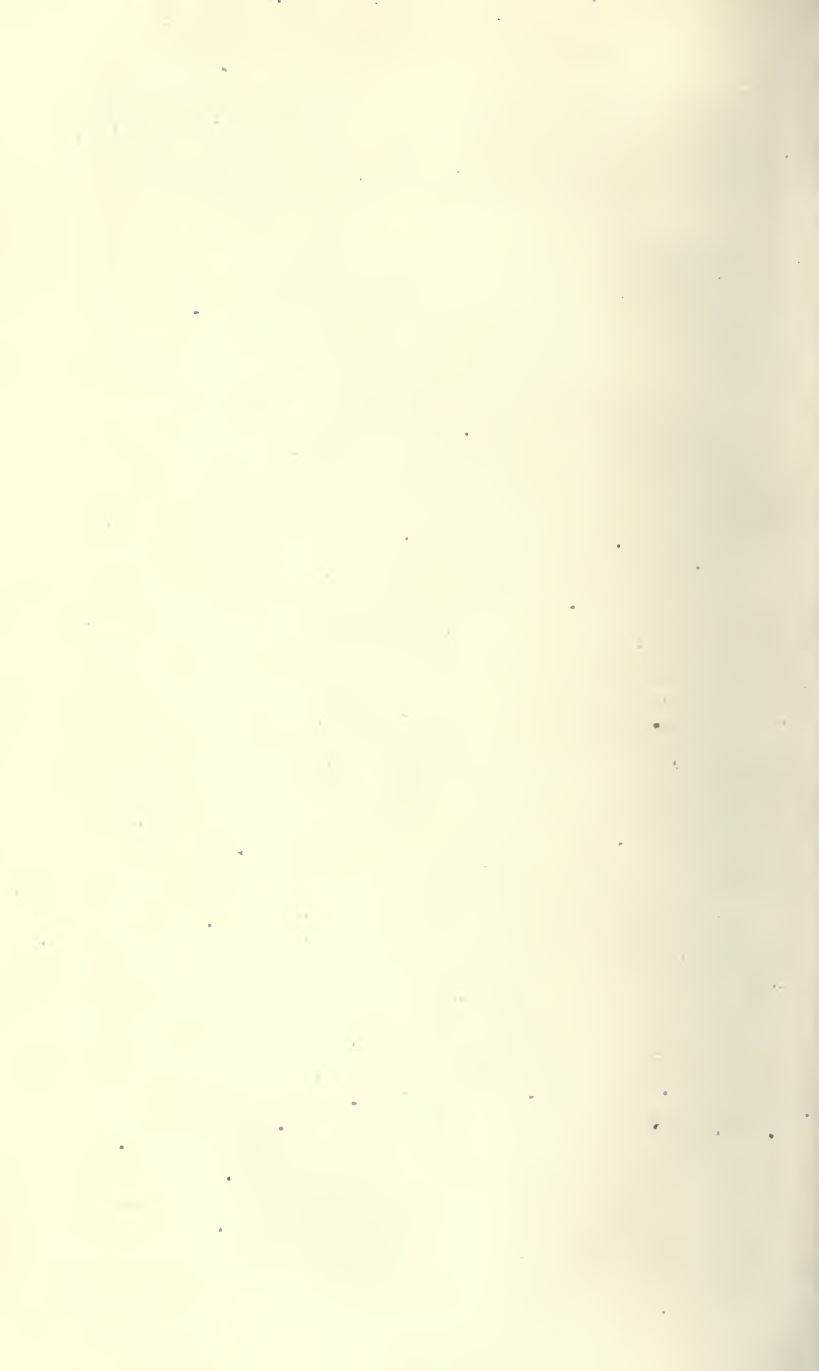
Foucault and Fizeau, in their well-known experiments on the interference of heat, no one has made reference to these lines, and that all experimenters describe the heat-curve as a continuous one (Phil. Mag. April 1872).

I may therefore be excused for remarking at this point, not only that the three lines in question were observed by me nearly thirty years ago, but that an engraving of them was published in the Philosophical Magazine, in a memoir announcing the discovery of fixed lines in the invisible portions of the spectrum (May 1843). It will be seen from an inspection of that engraving that the lines are marked α, β, γ. They were impressed on Daguerreotype plates by resorting to the well-known processes for obtaining photographs of the less-refrangible regions of the spectrum.

In view of the preceding statements and others that might be given, it may, I think, be affirmed that the general opinion held at the present day as to the constitution of the spectrum is this —that there exists a heat-spectrum in the less-refrangible regions, a light-spectrum in the intermediate, and a spectrum producing chemical action in the more refrangible regions. An experimental attempt to correct this view, and to introduce a more accurate interpretation of the constitution of the spectrum, will not be without interest, especially as it is necessarily and directly connected with the important subject of photometry. In this memoir I shall offer some experiments and suggestions respecting the heat of the spectrum, and in another, shortly to be published, shall consider the distribution of the so-called chemical rays. Among the numerous problems of actino-chemistry there are none more important than these.

All the experiments hitherto made on the heat of the spectrum have been on the principle of exposing a thermometer in the differently coloured spaces. Such was Sir W. Herschel's method. Leslie used a differential with small bulbs. Melloni, Müller, Tyndall, a thermoelectric pile, the form preferred being the linear. This was advanced successively through all the radiations, and the deflections of the multiplier noted.

Is not this method essentially defective? Does it not necessarily lead to incorrect results?

"There is an inherent defect in the prismatic spectrum, a defect originating in the very cause which gives rise to that spectrum itself—unequal refrangibility. Of two groups of rays compared together, one taken in the red the other in the violet region, it is clear that in the same spectrum, from the very circumstance of their greater refrangibility, those in the violet will be relatively more separated from each other than those in the red. The result of this increased separation in the more

refrangible regions is to give an apparent dilution to them, while the less-refrangible are concentrated. The relative position of the colours must also vary; the fixed lines must be placed at distances greater than their true distance as the violet end is approached." I am quoting from the fifth chapter of a work 'On the Forces which produce the Organization of Plants,' published by me in 1844. In this chapter one of the chief points insisted on is the necessity of using wave-lengths in the measurement and discussion of spectrum results—a suggestion which I believe I was the first to make, and which I renewed in the Philosophical Magazine, June 1845.

The importance of these remarks respecting the peculiarities of the prismatic or dispersion spectrum may perhaps be most satisfactorily recognized on examining such a spectrum by the side of a diffraction or interference one. By the aid of fig. 1 (Plate II.) this may be done.

Regarding the space between the fixed lines D and E as representing the central region in each, the fixed lines D and E are made coincident in both. The other lines are laid off in the prismatic spectrum as they appear through the flint-glass prism of the spectroscope; those of the diffraction spectrum are arranged according to their wave-lengths. It thus appears that in the prismatic spectrum from the fixed line D to A the yellow, orange, and red regions occupy but little more than half the space they do in the diffraction spectrum; while the green, blue, indigo, and violet from the fixed line E to H occupy nearly double the space in the prismatic that they do in the diffraction spectrum. The general result is that in the prismatic the less-refrangible regions are much compressed and the more refrangible much dilated. And it is plain that the same will hold good in a still greater degree for any invisible rays that are below the red and above the violet respectively.

Now, if a thermometer of any kind were carried in succession from the greatly dilated more refrangible regions to the greatly condensed less refrangible, could the measures obtained be accepted as expressing the true distribution? The thermometric surface being invariable, would it not receive in the less-refrangible spaces more than its proper amount of heat, and in the more refrangible less than its proper amount?

If we were to admit that the distribution of heat in a correctly formed spectrum is uniform, it is plain that measures made by the use of a prism would not substantiate that admission. The concentration to which I have alluded as taking place in the less-refrangible region would give an increased heat for that region; and, on the contrary, the dilatation of the more refrangible would give an exaggerated diminution of heat for that space. But if

it were possible to make satisfactory heat-measures on the diffraction spectrum, in which the coloured spaces and fixed lines are arranged according to their wave-lengths, the admission would be substantiated.

In view of these facts I did attempt many years ago to make heat-measurements on the diffraction spectrum; but so small is the heat, that, as may be seen in the Philosophical Magazine (March 1857), the results were unsatisfactory. More recently I have tried another method of investigation, on principles which I will now explain.

For the sake of clearness, restricting our thoughts for the moment to the more familiar case of the visible spectrum, if we desire to ascertain the true distribution of heat, would not the proper method be to collect all the less-refrangible rays into one focal group and all the more refrangible into another focal group, and then measure the heat that each gave? If the view currently received be correct, would not nearly all the heat observed be found in the former of these foci, and little, if indeed any, be found in the second? But if all the various regions of the spectrum possess equal heat-giving powers, would not the heat in each of these foci be the same?

Let us give greater precision to this idea. Using Ångström's wave-lengths, the length at the line A is 7604, that at H^2 3933; and these lines are not very far from the less and more refrangible ends of the visible spectrum respectively. The middle point of this spectrum is at 5768, which, therefore, may be called its optical centre. This is a little beyond the sodium-line D, which is 5892. Now, if by suitable means we reunite all the rays between 7604 and 5768 into one focus, and all the rays between 5768 and 3933 into another focus, are we not in a position to determine the true distribution of the heat? Should the heat at these two foci be sensibly the same, must not the conclusion at present held be abandoned?

If in these investigations the rays of the sun are used, it is necessary to restrict the examination to the visible spectrum, excluding the invisible red and invisible violet radiations. On these the earth's atmosphere exerts not only a very powerful but a very variable action, and what is still more, an action the result of which we cannot see, so that we are literally working in the dark. There are days on which, owing to the excessive absorption taking place among the ultra-red rays, a rock-salt train has no advantage over one of glass. But if it be the visible spectrum alone that we are using, and the prisms are of a material *colourless* to the eye, we may be certain that they are exerting no elective absorption on any of the radiations of that spectrum, and that the indications they are giving are reliable.

This variable absorptive action of the atmosphere depends partly on changes in the amount of water-vapour, and partly on the altitude of the sun. At midday and at midsummer it is at a minimum. The disturbance is not merely a thermochrose; for both ends of the spectrum are attacked. It is a matter of common observation that the horizontal sun has but little photographic power, owing to atmospheric absorption of the ultra-violet rays; and under the same circumstances his heating-power is diminished, owing to the absorption of the ultra-red rays. But if the day be clear and the sun's altitude be sufficient, the visible spectrum may be considered unaffected.

It should be borne in mind that the envelopes of the sun himself exert an absorptive action, which is powerfully felt in the ultra-violet region, as is indicated by the numerous fixed lines crowded together in that region. The force of this remark will be appreciated on examining the Plate above referred to, in the Philosophical Magazine for May 1843.

It seems, then, that all the conditions necessary for the solution of this problem will be closely approached if we make use of prisms constituted of any substance which is *completely colourless to the eye*, and confine our measures to the *visible spectrum*, collecting all the radiations between the fixed line A and the centre of the spectrum just beyond D into one focus, and all the radiations between that centre and H^2 into another focus, and, by the thermopile or any other suitable means, measuring the heat of these foci.

Such is the method I have followed in obtaining the measures now to be presented; but before giving them there are certain preparatory facts which I wish to submit to the consideration of the reader.

(1) In the mode of experiment hitherto adopted no special care has been taken to ascertain with accuracy the position of the "extreme red;" yet that is held to be the point from which on one side we are to estimate the visible, and on the other the invisible spectrum. Different persons, perhaps because of a different sensitiveness of their eyes, will estimate that position differently. The red light shades off gradually; it is almost impossible to tell where it really comes to an end. A linear thermopile, such as is commonly used, is liable under these circumstances to give deceptive results; and any error in its indications counts in a double manner: it not only diminishes the value of one spectrum, but it adds that diminution to the value of the other. The force of this remark will be understood by considering the best experiments hitherto made on this subject—those of Dr. Tyndall in his 'Heat a Mode of Motion' (London edition, 1870, p. 420 &c.). In the case of the electric light, the result yielded

by those experiments was that the heat in the invisible is eight times that in the visible region. But had there been an error in estimating the position of the extreme red by only two millimetres, so much would have been taken from the invisible and added to the visible that they would have been brought to equality, and then the slightest turn of the screw that carried the pile toward the dark space would have given a preponderance to the visible. It is obvious, therefore, that there cannot be certainty in such measures unless the fixed lines are resorted to as standard points.

(2) A ray which has passed through a solution of sulphate of copper and ammonia possesses no insignificant heating-power. I took a stratum of a solution of that salt of such strength that it only permitted waves to pass which are of less length than 4860. Seen in the spectroscope, the colours transmitted through it commenced with a thin green fringe, followed by blue, indigo, violet. It therefore gave rays in which, according to the accepted views, little or no heat should be detected. Yet I found that such rays produced one ninth of the heat of the entire solar beam. Does not this indisputably show that the more refrangible rays have a higher calorific power than is commonly imputed to them?

(3) Again, by the use of the apparatus presently to be described, I found no difficulty in recognizing heat in the violet region; but in the mode of conducting the experiment heretofore resorted to it could not be detected in rays more refrangible than the blue. It was this result which gave so much weight to the conclusion that in the more refrangible regions the calorific power is replaced by chemical force, and strengthened the idea commonly entertained that the solar radiations consist of three distinct principles—heat, light, and actinism. In the memoir above referred to as soon to be published, I shall present some facts which apparently make this view indefensible.

(4) If waves of light falling upon an absolutely black surface and becoming extinct thereby are transmuted into heat, if the warming of surfaces by incident light be nothing more than the conversion of motion into heat—an illustration of the modern doctrine of the correlation of forces, heat itself being only a "mode of motion"—it would seem extraordinary that this conversion should cease in the green or blue or in any ray. On the contrary, calorific effects ought to be traceable throughout the entire length of the spectrum. These views on the transference of motion from the æther to the particles of ponderable bodies, and conversely, I endeavoured to explain in detail in a memoir on Phosphorescence, inserted in the Philosophical Magazine, Feb. 1851, p. 98 &c. I had previously indicated them in the same

Journal, Feb. 1847. A given series of waves of red light impinging upon an extinguishing surface will produce a definite amount of heat, and a similar series of violet waves should produce the same amount; for though an undulation of the latter may have only half the length of one of the former, and therefore only half its *vis viva*, yet, in consequence of the equal velocity of waves of every colour, the impacts or impulses of the violet series will be twice as frequent as those of the red. The same principle applies to any intermediate colour; and hence it follows that every colour in the spectrum ought to have an equal heating-power.

Description of the Apparatus employed.

The optical arrangement I have employed for carrying the foregoing suggestions into practice is represented by fig. 2, and in a horizontal section by fig. 3.

A ray of sunlight reflected by a Silbermann's heliostat comes into a dark room through a slit a, 1 millim. wide. It then passes through a prism b. On the front face of this prism is a black paper screen, $c\,c$, having a rectangular opening just sufficient to permit the light of the slit to pass. After refraction the dispersed rays fall as a spectrum on a concave metallic mirror, $d\,d$, 9 inches in aperture and 11 in focus for parallel rays. I have sometimes used one of speculum-metal, but more frequently one of glass silvered on its front face. In front of this mirror there are therefore three foci. At a distance of eleven inches there is one, e, giving a spectrum-image of the sun. Still further there is a second, f, which is a spectrum-image of the slit a, in which, if the prism be at its angle of minimum deviation and the other adjustments be correctly made, will be seen the Fraunhofer lines. Again, still further off, at g, is a focal image of the rectangular opening of the black paper ($c\,c$) on the front face of the prism. This image, arising from the recombination of all the dispersed rays, is consequently white. These second and third foci are at distances from the mirror depending on the distances of the slit a and the black paper $c\,c$ respectively.

With the intention of being certain that the light coming through the slit a is falling properly on the rectangular opening in the prism screen $c\,c$, a small looking-glass is placed at p. The experimenter, sitting near the multiplier m, can then see distinctly the reflected image of that opening.

At the place where the second focal image with its Fraunhofer lines forms, two screens of white pasteboard (h, i) are arranged. By suitably placing the former of these, h, the more refrangible rays may be intercepted; and in like manner by the other, i, the less refrangible. In using these screens, and particularly h,

care must be taken that no rays passing from the prism to the mirror are obstructed—a remark that applies especially to the invisible rays of less refrangibility than the red. For this reason the mirror *d d* must be placed at such an obliquity to its incident rays as to throw the focal images sufficiently on one side. Yet this obliquity must not be greater than is actually necessary for that purpose, or the purity of the second spectrum with its Fraunhofer lines will be interfered with. At the place of the third focus, arising from the reunion of the dispersed rays, is the thermopile *g*, connected by its wires *k k* with the multiplier *m*.

Whenever any of the visible rays of the Fraunhofer spectrum are intercepted by advancing either of the screens *h* or *i*, the image on the face of the pile ceases to be white. It becomes of a superb tint, answering to the combination of the non-intercepted rays. A slip of white paper placed for a moment in front of the pile will satisfy the experimenter how magnificent these colours are. It is evident, therefore, that by this arrangement the pile will enable us to measure the heat of any particular ray or of any selected combination of rays. The screens can be arranged so as to reach any designated Fraunhofer line.

The pile I have used is of the common square form; a linear pile would not answer. The focal image on the pile is of very much greater width than the slit *a*, on account of the obliquity of the front face of the prism.

By removing the screen *h* and placing the screen *i* so that its edge coincides with the line A of the Fraunhofer spectrum, all the invisible heat-radiations of less refrangibility than the red are cut off, except the contaminating ones arising from the general diffusion of light by the substance of the prism. Under these circumstances the image on the pile will be white, and the multiplier will give a deflection representing the heat of the visual and the ultra-violet regions. If, then, the screen be advanced still further until it has intercepted all the less-refrangible regions up to the sodium line D, or a little beyond (that is, to the optical centre of the spectrum), the tint on the face of the pile will be greenish blue, and the multiplier will give a measure of the heat of the more refrangible half of the visible spectrum, together with that of the ultra-violet rays; the latter portion, however, may be eliminated by properly using the other screen, *h*.

Besides the error arising from stray heat diffused through the spectrum in consequence of the optical imperfection of the prism, there is another, which may be recognized on recollecting the relative positions of the prism, the concave mirror, and the face of the pile. It is evident that the prism, considered as a warm or a cool mass, is a source of disturbance, for the mirror reflects its image (that is, the image of the prism itself) to the pile.

After the intromitted sunbeam has passed through the prism for a short time, the temperature of that mass has risen; and the heat from this source has become intermingled with the proper spectrum heat.   But this error is very easily eliminated.   It is only necessary to put a screen *n* in the path of the incoming ray between the slit and the prism and note the deflection of the multiplier.   Used as we are here supposing, the multiplier has two zeros.   The first, which may be termed the magnetic, is the position in which the needles will stand when no current is passing through the coil.   The scale of the instrument should be set to this.   The other, which may be termed the working zero, is found by coupling the pile and the multiplier together and introducing the screen *n* between the intromitting slit and the prism.   On doing this it will probably be found that the index will deviate a few divisions.   Its position should be accurately marked at the beginning and close of each set of measurements, and the proper correction for them made.   The disturbing influences of the mass of the prism, of the mirror, and of the pile itself are thus eliminated.   As respects the last, it should not be forgotten that it may be affected by changes in the position of the person of the experimenter himself.

With the intention of diminishing these errors I have usually covered the upper and lower portions of the concave mirror *d d* with pieces of black paper, so arranged as to leave a band across the middle of sufficient width to receive and reflect the entire spectrum.   In fig. 4 *a a* is the upper paper, *b b* the lower, *c c* the uncovered reflecting band receiving the spectrum *r v*.   Had the spaces thus covered been permitted to reflect, they would have rendered more intense the image of the prism and its extraneous heat.

As regards the multiplier, care must be taken to avoid disturbance from aërial currents.   I have one of these instruments of French construction which could not be used in these delicate researches until proper arrangements were applied.   It was covered with a glass shade.   The slightest cause occasioned currents in its included air, which perpetually drifted and disturbed the needles.   For this reason, and also for more accurate reading, it is best to view the position of the index through a small telescope.

The combination of needles being nearly astatic, attention must be paid to their magnetic perturbations, whether arising from local or other causes; and since the vibrations are very slow, ample time must be given before the reading is ascertained.

The condition of the face of the pile is of importance.   It must be such as to extinguish as completely as possible all the incident rays.   To paint it with lampblack mixed with gum will

not answer; the surface so produced is too glossy and reflecting. The plan I have found best is to take a glass tube ½ an inch in diameter and 6 inches long, open at both ends, and use it as a chimney. A piece of camphor being set on fire at the lower end, and the face of the pile to be blackened being held for a moment at the upper, it is covered with a dense black film without any risk of injury to the pile. Even at the best, when this has been done, there is an unavoidable source of error in the want of perfect blackness of the lampblack. It is sufficient to inspect the face of the pile when receiving rays from the concave mirror to be satisfied how large a portion of light is reflected. The experiments of Dr. Tyndall show that this substance transmits a considerable percentage of the heat falling on it. Its quality of transmitting light is well known to every one who has looked at the sun through a smoked glass.

The galvanometer I have used is calibrated according to the usual method; the numbers given in this memoir do not represent the angles of deflection, but their corresponding forces.

The proper position of the intercepting screens h, i can often be verified with precision by looking through a blue cobalt glass. This glass insulates a definite red, an orange, and a yellow ray in the less-refrangible regions, and then, commencing with the green, gives a continuous band to the end of the violet. Its red ray begins at the less-refrangible end of the spectrum, and ends near C. It includes the fixed lines A, B, C. Its orange ray lies wholly between C and D, including neither of those lines. Its yellow ray begins near 5894, and ends about 5581; the line D is therefore near its point of beginning. Its end is about halfway from D to E. The remaining continuous band begins about 5425; it therefore includes the lines E, b, F, G, H. I have found this glass of much use in determining how far the screen i has been pushed. It is convenient to select a light kind of it; and by looking through one, two, or three pieces the depth of colour can be regulated at pleasure.

The optical train which has thus acted on the sunbeam under examination is therefore (1) the sun's atmosphere, (2) the earth's atmosphere, (3) the heliostat-mirror of speculum-metal, (4) the prism, (5) the concave mirror of silvered glass, (6) the blackened face of the thermopile.

Results obtained by the Apparatus.

We are now ready to examine the results which this optical apparatus yields, it having been, of course, previously ascertained that the reflecting band of the concave mirror $d\,d$ is sufficient to receive all the radiations coming from the prism, and that none are escaping past its edges.

The operations now required are as follows:—

The heliostat is to be set and its reflected ray brought into the proper position. The optical train is adjusted, the prism being at its minimum deviation, and the concave mirror giving a white image on the face of the pile.

The screen h is then to be placed so that, without intercepting any rays coming from the prism to the mirror, it cuts off all in the Fraunhofer spectrum above H^2.

The screen i is so placed as to cut off all rays less refrangible than the sodium-line D. More correctly this screen should be a little beyond D. The light on the face of the pile will now be greenish blue.

The screen n is then so placed as to intercept the intromitted beam. When the needles of the multiplier come to rest they give the working zero, which must be noted.

The intromitting screen n being now removed, the multiplier will indicate all the heat of the more refrangible rays—that is, from a little beyond D up to H^2. The force, corrected for the working zero, is to be noted.

The screen i is then removed to the line A, so as to give all the radiations between the lines A and H^2. The light on the face of the pile is white, and the multiplier gives the whole heat of the visible spectrum. By subtracting the foregoing measure from this, we have the heat of the less-refrangible region—that is, from A to the centre of the spectrum.

As a matter of curiosity the experimenter may now, if he pleases, remove the screens h, i; the light on the face of the pile will still be white, and the multiplier will give the force of the entire radiations, except so far as they are disturbed by the thermochrose of the media. These measures, as not bearing upon the problem under consideration, I do not give in the following Tables.

Instead of advancing the screen i from the less toward the more refrangible regions, I have very frequently moved h from the more refrangible regions toward the less. When it is brought down from H^2 to the centre of the spectrum, the light on the face of the pile is of an intense orange-red; it might perhaps be called a bromine-red. I need not give further details of this mode of experimentation, as I did not find that its results differed in any important degree from those obtained as just described.

The variation in different experiments may generally be traced to errors in placing the screen i with exactness on the centre of the spectrum and on the line A.

For the sake of more convenient comparison I have reduced all the different sets of experiments to the standard of 100 for the whole visible spectrum.

I have made use of four prisms :—(1) rock-salt, (2) flint-glass, (3) bisulphide of carbon, (4) quartz, cut out of the crystal so as to give a single image. All the observations here recorded were made on days when there was a cloudless sky.

Table I.—Distribution of Heat by Rock-salt.

	Series I.	Series II.
(1) Heat of the whole visible spectrum .	100	100
(2) Heat of the more refrangible region .	53	51
(3) Heat of the less refrangible region .	47	49

In this Table the column marked Series I. gives the mean of four sets of measures, and that marked II. of three. At the beginning of each set the rock-salt was repolished.

Table II.—Distribution of Heat by Flint-glass.

	Series I.	Series II.
(1) Heat of the whole visible spectrum .	100	100
(2) Heat of the more refrangible region .	49	52
(3) Heat of the less refrangible region .	51	48

Series I. gives the mean of ten sets of measures, Series II. of eight.

Table III.—Distribution of Heat by Bisulphide of Carbon.

	Series I.	Series II.
(1) Heat of the whole visible spectrum .	100	100
(2) Heat of the more refrangible region .	52	49
(3) Heat of the less refrangible region .	48	51

The sulphide employed was devoid of any yellowish tinge. It was quite clear. Series I. is the mean of eight experiments, Series II. of ten.

Table IV.—Distribution of Heat by Quartz.

	Series I.	Series II.
(1) Heat of the whole visible spectrum .	100	100
(2) Heat of the more refrangible region .	49	53
(3) Heat of the less refrangible region .	51	47

Series I. represents twenty-seven experiments, Series II. twelve. In the former two quartz prisms were used to increase the dispersion ; in the latter only one was employed.

Perhaps it may not be unnecessary for me to say that I have repeated these experiments many hundred times during a period of several months, including the winter and the summer, varying the conditions as to the hour of the day, arrangement of the apparatus, &c. as much as I could, and present the foregoing Tables as fair examples of the results. Apprehending that the

heliostat-mirror, which was of speculum-metal, might exert some disturbing influence on account of its faint reddish tinge, I replaced it with one of glass silvered on the front face, but could not detect any substantial difference in the results.

The important fact clearly brought into view by these experiments is, that, if the visible spectrum be divided into two equal portions, the ray having a wave-length of 5768 being considered the optical centre of such a spectrum, those portions will present heating-powers so nearly equal that we may impute the differences to errors of experimentation. Assuming this as true, it necessarily follows that any two series of undulations in the spectrum will have the same heating-powers, no matter what their wave-lengths may be.

But this conclusion leads unavoidably to a most important modification of the views now universally held as regards the constitution of the spectrum. When a ray falls on an extinguishing surface, heat is produced; but that heat did not preexist in the ray. It arose from the stoppage of the æther waves, and is a pure instance of the conversion of motion into heat—an illustration of the modern doctrines of the conservation and transmutation of force.

From this point of view the conception that there exist in an incident ray various principles disappears altogether. We have to consider an incident ray as consisting solely of æthereal vibrations, which, when they are checked by an extinguishing substance, lose their *vis viva*. The effect that ensues depends altogether on the quality of this substance. The vibrations imparted to it may be manifested by the production of heat, as in the case of lampblack; or by chemical changes, as in the case of many of the salts of silver. In the parallel instance of acoustics, clear views have long ago been attained and are firmly held. No one supposes that sound is one of the ingredients of the atmosphere, and it would not be more incorrect to assert that it is something emitted by the sounding body than it is to affirm that light or heat or actinism are emitted by the sun.

The progress of actino-chemistry would be greatly accelerated if there could be steadfastly maintained a clear conception of the distinction between the mechanism of a ray and the effects to which that ray may give rise. The evolution of heat, the sensation of light, the production of chemical changes, are merely effects—manifestations of the motions imparted to ponderable atoms. And these in their turn can give rise to converse results—as, when we gradually raise the temperature of a substance, the oscillating movements of its molecules are imparted to the æther, and waves of less and less length are successively engendered.

In the title of this memoir I have employed the phrase "Distribution of Heat" in accordance with general usage; but if the conclusion arrived at be true, it is plain that this should be exchanged for "Production of Heat." The heat observed did not preexist in the incident rays, it is the result of their extinction.

The remark has been made that these results are essentially connected with photometry. In fact, any thermometer is converted into a photometer if its bulb or other receiving surface be coated with a perfectly opaque non-reflecting substance.

XIII. *A necessary Correction of one of* Mr. Tait's *Remarks.* By R. Clausius*.

SINCE I refuted the objections made in a previous article by Mr. Tait against my treatment of the mechanical theory of heat, in a second article he raises new objections. These would be just as easily invalidated; but the tone in which Mr. Tait has written renders it impossible for me to continue the discussion.

There is one point, however, which I *must not* leave uncorrected, because it relates to a third party, who is highly esteemed alike in England and in Germany—namely, Sir W. Thomson, whom Mr. Tait mentions in such a way as if I had made a charge against him: he asks me if I seriously suppose that Thomson has been deliberately attempting to deprive me of my just claims. Of Sir W. Thomson, however, I have not said, or even remotely hinted, any thing of the kind. Quite the contrary: I have expressly called attention to his frank recognition of my works. Mr. Tait himself, at the close of his article speaks quite differently of the relation between Sir W. Thomson and myself—where, in order to countenance his own attacks upon me, he says:—"and I think that Thomson has done mischief as regards scientific history by giving Professor Clausius undue credit." This makes the first-mentioned passage concerning Thomson (which is hardly a page distant) so much the more incomprehensible; and it is a note-worthy example of the way in which Mr. Tait discusses existing facts.

The rest of the article I simply leave to the judgment of those readers who will take the trouble to compare Mr. Tait's various statements with one another and with my memoirs. Even if an article of a similar kind were to follow, I should decline to answer it; but in that case I beg the readers of the Philosophical Magazine not to interpret my silence as assent.

Bonn, July 1872.

XIV. *The Atomic Theory, in Reply to* Dr. Wright. *By* R. W. Atkinson, *F.C.S.*[*]

IT is exceedingly gratifying to find that Dr. Wright gives a practically unqualified assent to the Atomic Theory. He attempts to distinguish between the atomic hypothesis and the atomic theory; but when he defines a theory to be a proposition the predictions of which are "mostly verified by experiment and observation," it will be seen that, even according to his own definition, the term theory is more applicable than hypothesis to Dalton's statement. Now, as he in no place attempts to criticise what he terms the "atomic theory (as employed in University College laboratory)," and as it can be shown that this view is merely a further development of Dalton's original views, we may fairly accept the weight of Dr. Wright's opinion in favour of the theory.

Dalton represented elementary bodies as made up of an assemblage of globules, each of which consisted of a central atom of solid matter surrounded by an atmosphere of heat. The only difference between this view and that now accepted by chemists, apart from the difference in the theory of heat, is that they do not commit themselves to any assertion as to the nature of this central atom,—certainly not as great a modification as adopting the mechanical in place of the material theory of heat, which no one would regard as altering the main features of Dalton's conception. Chemists treat such atoms in the same way as astronomers treat the planets—that is, as practically indivisible, although no astronomer would assert that they are in their nature indivisible: the question is not raised.

By his analysis of Dalton's proposals into generalization, convention, and hypothesis, Dr. Wright has not escaped from the dilemma in which he was placed with regard to the combination of elements in definite and multiple proportions. If this threefold division had been real, it would have afforded a very fair means of escape; but the three propositions are so inextricably bound up together that it is not possible to separate any one from the rest. Dr. Wright represents "the quantitative composition of any homogeneous body . . . . by taking and comparing simple multiples of certain fixed numbers attached respectively to the name of each elementary substance occurring in the body in question;" in other words, he attaches to a concrete body a pure or abstract number; but under such circumstances the number itself also becomes concrete, in which case the unit to which it must be referred can only be a portion of the homogeneous matter to which it is attached. What benefit, then, can

accrue by concealing from oneself the fact that this is only a clumsy expedient for getting rid of the *word* atom ? the essential idea conveyed is that of an indivisible portion of matter, *i. e.* an atom. Thus, then, the generalization being bound up with the hypothesis, it follows that the convention is also bound up with them, the symbols and suffixes merely being used as an abbreviation for such an expression as " one atom of chlorine weighing 35·5 times an equal volume of hydrogen combined with one atom of hydrogen weighing 1," indicated by the formula HCl. Hence Dr. Wright has not shown that symbols can be used to represent the quantitative composition of bodies without involving the notion of atoms. If, as is done by Dr. Wright, symbols be used to represent elements, and suffixes to indicate the multiple of some fixed number mentally associated with that symbol, it is merely putting the notion of atoms in a form of speech which is calculated to produce the erroneous impression that that idea has been dispensed with.

Having thus shown, first, that the atomic theory now in use is only a development of Dalton's original conception and is perfectly consistent with it, and, secondly, that the notion of atoms is involved in Dr. Wright's account of Dalton's convention, his objections to my remarks can be easily disposed of.

The first objection has already been noticed. The generalization and convention cannot be separated from the theory which Dalton proposed, they are so completely interwoven. But, from Dr. Wright's remark, it would seem useless to discuss this point further.

The next objection is that, if my line of argument were correct, it would be impossible to assign any formula to any thing ; Dr. Wright omits to add the condition which was attached, viz. that the notion of atoms be not held. Without that belief one has no right to take the numbers corrected by the atomic theory in preference to those obtained by experiment. In speaking of the law of multiple proportions, Dr. Wright must mean either multiples of equivalent weights or of atomic weights. If the latter, as it would appear, how much more certain are they than the atomic weights of which they are the multiples ?

Respecting the ". combining number " of aluminium, the selection of which Dr. Wright thinks was very unfortunate, the determination of the vapour-density of aluminic chloride by Deville must be well known, which shows that its formula is $Al^2 Cl^6$. With regard to Buckton and Odling's determination of the vapour-densities of aluminic methide and ethide, the results are totally at variance with those obtained in the case of other similar compounds, and, further, have never been accepted by chemists.

If Dr. Wright had read my paper carefully, he would have seen

that his exception of oxygen and chlorine on account of difference of physical condition was referred to. In a note he objects to my taking the specific heat of the gases under constant volume instead of under constant pressure; but that, I maintain, is the correct number to take, inasmuch as the excess of heat in the latter case is only used in lifting the atmosphere through a certain space.

Dr. Wright says that boron and carbon were not mentioned in connexion with the rule; but why are two elements of such importance to be neglected? Does he, then, throw on one side every thing which does not happen to coincide with his views?

It is not worth while discussing Dr. Wright's repetition of the statement that valency is a function of symbols; valency being a property possessed by matter, cannot be merely a function of certain symbols.

Instead of accounting for the differences between isomeric bodies, Dr. Wright evades this by making a furious attack upon the atomic theory respecting the validity of its own explanation. All that his argument amounts to, however, is this, that the relation between the motion of the atoms and the mechanical properties of a body has not been studied. Until this is the case it is absurd to expect the atomic theory to state what difference in the motion of the atoms corresponds to a difference in the mechanical properties of a body. The reactions of ethylic alcohol and methylic ether are satisfactorily explained on the supposition that in the first case an atom of oxygen binds together an atom of ethyle and an atom of hydrogen, and in the second case it binds together two atoms of methyle; but any connexion between the motion of the atoms and the mechanical properties of bodies must be left for future researches to point out. It is no argument against the atomic theory to say that it does not account for certain properties, when it has never been applied to their explanation. It will be time enough to cry out against it when it is found perfectly incompetent to do so after the requisite knowledge has been obtained. But if that explanation be rejected on account of its alleged insufficiency, what is to take its place? for it is evident, from the care with which he evades the question proposed, that Dr. Wright has none to offer.

University College Laboratory,
    July 13, 1872.

XV. *Remarks on the alleged ambiguity, insufficiency, and unne-cessariness of the Atomic Theory.* By ALFRED TRIBE[*].

IT has been observed by a great thinker "that those who con-sider it simply the part of science to record results of ob-servations, and not to endeavour to connect them with one another, know not what science is." It may further be remarked that facts, without some means of connecting them, do not con-stitute science. The only means known at present by which the numerous facts relating to the transformations of matter can be connected is by what is generally termed the atomic theory. It matters little whether this fact-connector and mental help be called theory, hypothesis, or the "invention of a mechanical or carpenter's mind;" there is no ignoring the historical facts that its discovery threw light where utter darkness prevailed, and that the vivid pictures (always in harmony) it enabled the mind to create acted as a wonderful stimulus to the chemical explorer—so much so, that the progress of his science prior to and since its enunciation may well be compared to the sluggish flow of the rivulet and the rapid motion of the mountain-torrent.

The service which this simple but noble creation of a great mind has done, is doing, and is destined to do induces me to venture a few brief remarks on its alleged ambiguity, insuffi-ciency, and unnecessariness.

Ambiguity.—The theory is ambiguous, according to Dr. Wright, because "the phraseology of the hypothesis of the existence of atoms is employed by chemists in different senses." Were this more than the expression of an opinion based on isolated state-ments of a few chemists, I should say at once lay the axe to the tree that bears such evil fruit. But I should be glad to know what there is in this theory necessarily productive of ambiguity. Is it beyond the power of ordinary intellects to comprehend? I know as a fact that youths of ordinary intelligence, from about the ages of 14 to 18, experience no difficulty in understanding its nature and its bearings. If teachers differently distort the theory and their pupils accept these distortions, ambiguity may result when the latter use its phraseology; but this is the fault of wrong teaching, not of the theory, and permits of ready rec-tification by a pilgrimage to the fountain-head.

Insufficiency.—Dr. Wright admits that several of the laws and generalizations of chemistry are in accordance with the atomic theory. He charges it, however, with insufficiency on the grounds that "it gives no clue to the explanation of the remark-able approximate relationships existing between the numerical values of the atomic weights, nor to the remarkable sequence in

* Communicated by the Author.

which those elements at present known follow each other," and that, "to explain Dulong and Petit's law, the assumption must be made that the effect of a given quantity of heat in raising the temperature of a solid mass containing a given number of elementary atoms all of the same kind is independent of the nature of the atoms." Dr. Wright has collected a number of facts on which the philosophy of chemistry is based; and his essay is useful on that account; but the analysis of these charges of insufficiency demonstrate that, in his zeal, he lost sight of the aim of the theory, and endeavoured to make it answerable for phenomena with which it has no necessary connexion. These "remarkable approximate relationships" and this "remarkable sequence" are facts of some small interest. It is, however, no more the part of the atomic theory even to suggest their cause, than it is to explain why atoms of different substances differ in weight, or why chlorine and bromine, possessing, as they do, so many chemical analogies, should be respectively a yellowish-green gas and a reddish-brown liquid.

Dulong and Petit's law is the expression of a really remarkable series of experimental facts, and has been instrumental in no small degree in increasing the faith in the existence of atoms. But why this argument against the idea of the atomic constitution of matter because quantities of certain elementary bodies expressed by their atomic weights require the same amount of heat to raise them to a given temperature, I am at a loss to understand. Had more than a superficial view of this subject been taken, the discovery would have been made that it is the atomic theory which welds Dulong and Petit's results into a law—that, in fact, the law exists in virtue of the theory, and consequently that the theory is independent of the law. Even were it a fact that atomic proportions of the sixty-four known elementary bodies required as many different amounts of heat to raise them to a given temperature, the fact could not be employed as an argument for or against the atomic theory.

If theories are to be charged with being insufficient on grounds no more potent than have been brought against the atomic theory, the theory of gravitation may fairly be expected to be charged, at no distant day, with insufficiency because it gives no clue to the cause of sun-spots or the physical constitution of the sun; the undulatory theory, because it gives no explanation of the disinfecting action of burning brimstone; and the theory of the dissipation of energy, because it fails to account in a satisfactory manner for the odour peculiar to *Sauerkraut*.

Unnecessariness.—Dr. Wright remarks, "some minds are unable to grasp the notion that, for example, 6 and 8 are 14 without going through the mechanical process of counting on the

fingers." As the comparative amounts of mental exertion required for these operations remain undetermined, it is an open question which requires the greater; but the fact that Dr. Wright compares the mental help which is afforded by a theory to that of the mechanical process of counting on the fingers, explains in a more forcible manner than I can why he charges the atomic theory with being unnecessary.

It appears that Dr. Wright would have the man of science erect his edifice with facts only; but I imagine, were he a builder, he would wish for something more than bricks. Also that he would deprive the investigator of using the power of imagination—one of the greatest possessions of an intellectual being; but were he to examine the work which has been done in chemistry and physics, above the grade of the artisan, he would find such evidence of the value of theory or the power of imagination that he, whom I know to be actuated by love for our science, would be sorry to see chemists debarred from the legitimate use of their mental instrument of research.

Were the anti-atomists to attack the object of their aversion by experiment, no one could doubt their sincerity, and in all probability much good would result to science; but to attack any theory in the worn-out style of certain writers prior to the time of Bacon, not only is productive of waste of energy, but may give rise to the opinion among science-readers that the men who know how to employ and keep a theory in its proper place necessarily believe in its being true.

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XVI. *Earthlight on the Moon. By* N. S. Shaler, *Professor of Palæontology, Harvard University.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

YOUR correspondent, Mr. Samuel Sharpe, in a note " On the Moon seen by the naked Eye," in the Number for June, seems to have fallen into an old and often-corrected error concerning the origin of the faint light which may be seen over the dark part of the moon during the time when the bright part is limited to a narrow crescent*. That ashy light, " *lumière cendrée*," as it has been called by the French, is clearly caused by the earthshine or light of the sun-lit earth reflected back to us. It would hardly be worth while to call attention once again to one of the simplest of the phenomena of our satellite, were it

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* The true cause of this appearance was first recognized by Leonardo da Vinci, and afterwards by Mœstlin.

not that it gives a chance to say something concerning the appearance of this dark surface at this state of illumination when seen through a telescope of first-class light-gathering power. With the 15-inch Mertz of the observatory of this University it is possible, under favourable conditions, to see all the principal features of the topography on the dark region illuminated only with this earthshine. In the course of some years of study upon the geology, if we may so call it, of the moon, I have had several opportunities of seeing, under these conditions, all the great features of the dark surface shine out with amazing distinctness. The curious point, however, is that the eye is not enabled to recognize the craters by light and shade, for the light is too feeble for that, besides being too vertical for such a result ; but the relief is solely due to the difference in the light-reflecting power of the various features of the topography. Whatever becomes very brilliant under the vertical illumination of the full moon (the edges and floors of many craters, certain isolated hills, and the radiating bands of light) shines out with a singular distinctness when lit by our earth's light. This is important, inasmuch as it shows pretty conclusively that the differences in the brightness of various parts of the surface of the moon is not due to the effects of the heating of the surface during the long lunar day, but is dependent upon difference in the light-reflecting power. There are several degrees of brightness observable in the different objects which shine out by the earth-light. In this climate there are not over three or four nights in the year when the moon can be caught in favourable conditions for this observation. The moon should not be over twenty-four hours old * (the newer the better), and the region near the horizon should be reasonably clear. Under these conditions I have twice been able to recognize nearly all the craters on the dark part over fifteen miles in diameter, and probably one half the bands which show with a power of 100 when the moon is full.

That this partial illumination of the dark part of the moon is in one way connected with the action of an atmosphere is clearly shown by the fact that the light is evenly distributed over the whole surface, and does not diminish as we go away from the part which is lit by direct sunlight, as it should do if the supposition of Mr. Sharpe were well founded.

It will be noticed that this fact probably explains the greater part of the perplexing statements concerning the illumination of certain craters before the terminator came to them. It certainly accounts for the volcanic activity which has so often

* Sometimes when the moon is two days old, and the other conditions are favourable, this illumination is faintly visible.

been supposed to be manifested by Aristarchus. Under the illumination of the earthlight this is by far the brightest object on the dark part of the moon's face, and is visible much longer and with poorer glasses than any other object there.

Truly yours,

N. S. SHALER.

Harvard University, Cambridge,
  Massachusetts, U.S.A.

XVII. *A Contribution to our knowledge of Atmospheric Waves.* By W. R. BIRT, *F.R.A.S., F.M.S.*[*]

THE issue of weather-charts by the Meteorological Office places at the disposal of such amateurs as may be willing to subscribe for them very valuable data bearing on the general affections of the atmosphere, temperature, pressure, wind, &c. Having given some little attention in former years to the subject of atmospheric waves, I have looked into the weather reports and charts for evidence bearing on these movements, and have succeeded in finding some important corroborations of the views I announced more than a quarter of a century ago, as well as some of a novel nature which the data then in my possession were unable to elucidate.

In the interval that has elapsed between the present and my former researches, a most important generalization has been arrived at; it is that, whatever may be the direction of the wind, the region of *lowest* pressure is on the *left* of this direction, and the region of *highest* pressure on the *right*. Observation shows that winds are arranged in two ways—cyclonic and linear; and in both, this law, known as Buys Ballot's law, holds good. If any wind of a linear arrangement be considered in relation to pressure, places on the left of the line of this wind will have lower pressures, with either the *same* wind or winds appertaining to the slope of a wave coexistent with that of which it forms a part, but which is moving in a different direction; and places on the right of the line of wind will have higher pressures, also with the same wind or other winds, as in the case just alluded to. The region between the lines of highest and lowest pressures is considered as being under the slope of an atmospheric wave (see figs. 1 and 2).

Between a *true* linear and a *true* cyclonic arrangement of wind every variety of curvilinear motion, from a very slight to a very decided curve, may exist. By the air flowing into a cyclone in a direction contrary to that of the hands of a watch the upward movement is continually fed, while the outflow from a region of high pressure is in the opposite direction, or with the hands of a

watch. Although this movement partakes of a cyclonic character, in order to distinguish it from that of a true cyclone, which circulates around a region of depression, it has been termed *anticyclonic,* as circulating in a reverse direction around a region of elevated pressure.

Fig. 1.

		STRONG WIND	POSTERIOR TROUGH.		
WIND	*a* S. W. S. E. *a'*	BAROMETER FALLING.	POSTERIOR SLOPE.	*a* S. W. S. E. *a'*	WIND
		CALM.	CREST.		
WIND	*b'* N. E. N.W. *b*	BAROMETER RISING.	ANTERIOR SLOPE.	*b'* N. E. N.W. *b*	WIND
		STRONG WIND.	ANTERIOR TROUGH.		
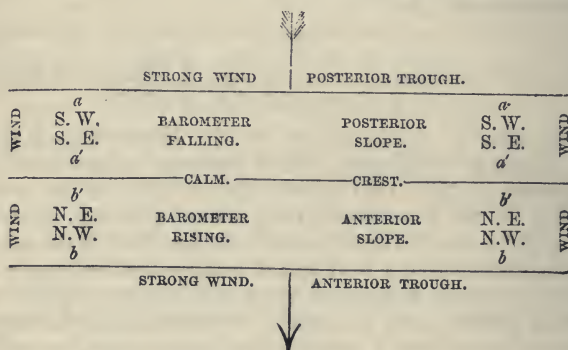
Fig. 2.
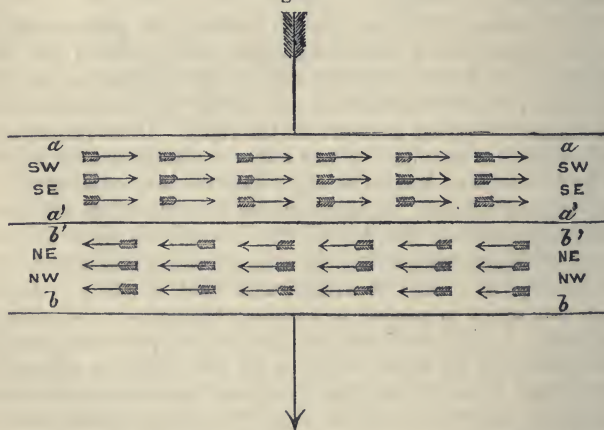
On the 19th of April, 1872, the weather-charts show a stream of air along the eastern coast of Great Britain, direction N.N.W., curving over the eastern counties of England and crossing the channel as a N.E. wind. The isobars or lines of equal pressure show the highest barometers in Ireland, and the lowest in the south of France. The gradations of pressure can be arranged in a Table in accordance with the following enunciation.

The barometric heights and corresponding winds, as shown on the charts for each day, may be resolved into two sets, the intersecting angles approaching more or less to right angles; and these heights and winds may be exhibited in Tables showing the decrease or increase of pressure from regions of high to low barometers, and the reverse.

Diurnal isobars, which are simply lines of equal pressure resulting from existing natural arrangements, do not exhibit any meteorological entity. To find such natural arrangements it is necessary to resolve the isobars and wind-curves into their barometric and anemonal elements.

TABLE I.—Distribution of Pressure and Wind over the Northwest of Europe on April 19, 1872. Crest of S.W. wave probably near Valencia.

First or highest zone below the crest.   S.W. *wave.*

	Barom.	Direction.	Force.
Valencia . . .	30·18	E.N.E.	4
Corunna . . .	29·77	N.E.	6

Second zone below the crest.   S.W. *wave.*

Roche's Point . .	30·03	N.N.E.	4

Third zone below the crest.   S.W. *wave.*

Greencastle . .	30·09	N.N.W.	2
Scilly	29·98	N.E.	5
Biarritz	29·67	E.	3

Fourth zone below the crest.   S.W. *wave.*

Holyhead . . .	30·03	N.N.E.	5
Pembroke . . .	30·03	N.E.	3
Plymouth . . .	29·96	E.N.E.	3
L'Orient . . .	29·87	N.E.	4
Rochefort . . .	29·77	E.	5

Fifth zone below the crest.   S.W. *wave.*

Ardrossan . . .	30·08	N.W.	2
Liverpool . . .	30·03	N.N.E.	3
Portsmouth . .	29·92	N.E.	3

Sixth zone below the crest.   S.W. *wave.*

Leith	30·05	N.E.	3
London	29·91	N.N.E.	3
Dover	29·83	N.N.E.	4
Cape Gris Nez .	29·82	N.E.	7
Paris	29·82	N.N.E.	2
Charleville . . .	29·80	E.N.E.	3
Lyons	29·69	N.	1
Toulon	29·63	N.E.	4

Seventh zone below the crest. S.W. *wave.*

	Barom.	Direction.	Force.
Thurso	30·03	N.	3
Wick	30·03	N.	3
Nairn	30·07	N.E.	3
Aberdeen	29·97	N.N.W.	6
Shields	29·98	N.E.	5
Scarborough	29·93	N.	5
Yarmouth	29·87	N.N.W.	4
Brussels	29·79	N.N.W.	4

Eighth zone below the crest. S.W. *wave.*

Helder	29·79	? S.	2

The regular decrements of pressure in each zone are in accordance with the anterior slope of a N.W. wave, the crest of which was not far from Valencia, Greencastle, and Nairn.

TABLE II.—Distribution of Pressure and Wind over the North-west of Europe on April 20, 1872, exhibiting a diminution of pressure and consequent production of the posterior slope of a S.W. wave.

Zone on S.W. *next above trough of* S.W. *wave.*

	Barom.	Direction.	Force.
Valencia	29·59 − 0·59	*N.	+7
Corunna	29·67 − 0·10	N.	+9

Trough of S.W. *wave.*

| Roche's Point. | 29·46 − 0·57 | *N.N.E. | 4 |
| Biarritz | 29·40 − 0·27 | Calm. | |

First zone above trough. S.W. *wave, posterior slope.*

Scilly	29·64 − 0·34	*S.S.W.	−3
L'Orient	29·68 − 0·19	S.E.	−2
Rochefort	29·54 − 0·23	N.E.	5

Second zone above trough. S.W. *wave, posterior slope.*

Greencastle	29·53 − 0·56	*E.	+5
Pembroke	29·62 − 0·41	*S.	3
Plymouth	29·67 − 0·29	E.S.E.	1

Third zone above trough. S.W. *wave, posterior slope.*

| Holyhead | 29·62 − 0·41 | *S.S.E. | −3 |

* The winds marked (*) thus are cyclonic.

Fourth zone above trough. S.W. *wave, posterior slope.*

	Barom.	Direction.	Force.
Ardrossan . .	29·66—0·42	E.S.E.	+5
Liverpool . .	29·69—0·34	S.E.	−2
Portsmouth .	29·71—0·21	N.E.	3

Fifth zone above trough. S.W. *wave, posterior slope.*

	Barom.	Direction.	Force.
Thurso . . .	29·79—0·24	E.N.E.	−2
Wick . . .	29·81—0·22	E.S.E.	3
Nairn . . .	29·76—0·31	E.	+2
Aberdeen . .	29·77—0·20	N.N.W.	−2
Leith . . .	29·73—0·32	S.E.	3
Shields . . .	29·73—0·25	S.	−2
Scarborough .	29·74—0·19	S.S.E.	−1
Yarmouth . .	29·75—0·12	N.E.	4
London . .	29·73—0·18	E.N.E.	3
Dover . . .	29·71—0·12	E.N.E.	4
Cape Gris Nez.	29·72—0·10	E.	−4
Brussels . .	29·72—0·07	N.E.	3
Paris . . .	29·65—0·17	N.N.E.	2
Charleville .	29·66—0·14	N.E.	3
Lyons . . .	29·55—0·14	N.	+3
Toulon . . .	29·44—0·19	E.N.E.	+7

The column of barometric differences shows the rise or fall of the mercury since the preceding day, while the + and − signs in the column of force indicate an increase or decrease of strength as compared with that of the preceding day.

While a general fall of the barometer was recorded at every station, it was greatest in Ireland and least in Belgium—a result obtained in my former researches. This fall could not have resulted from the progression of the waves; for we find the progression of both well marked, the crest of the N.W. wave extending along the English Channel and that of the S.W. wave along the eastern shores of Great Britain; yet along each crest the barometer had fallen. The fall must have resulted from an *irregular* change in the elasticity of the air, which commenced most probably near the western shores of Ireland, where the diminution was greatest. In this locality it assumed the form of a cyclone, not of very great force, but still well marked so far as the circulation of the wind was concerned. The cyclonic winds are distinguished in the Table by an asterisk (*). There are one or two note-worthy features in the Table, particularly the approach to an equality of pressure in that part of the fifth zone extending from Thurso to Brussels, in which portion we have the highest pressure (29·81) at Wick with a fall of 0·22 since the

19th, and the lowest (29·71) at Dover with a fall of 0·12.   The fall at Brussels was 0·07 only, the barometer standing at 29·72.   Another note-worthy feature is the synchronous preservation of the wave-form with the general decrease of pressure, a feature of great importance as bearing on the principles of storm-prevision.

Table III.—Distribution of Pressure and Wind over the North-west of Europe on April 21, 1872.

During the twenty-four hours, from 8 a.m. of the 20th to 8 a.m. of the 21st, the relations of the crests and troughs of both the S.W. and N.W. waves were reversed; the localities of high barometers on the 20th were those of low barometers on the 21st, except at the N.W. stations.

First zone on N.E. *above trough of* S.W. *wave, posterior slope.*

		Barom.	Direction.	Force.
Brussels	. .	29·22 − 0·50	*E.	3

Trough of S.W. *wave.*

		Barom.	Direction.	Force.
Thurso	. . .	29·81 + 0·02	N.N.E.	+4
Wick	. . .	29·82 + 0·01	E.N.E.	+5
Nairn	. . .	29·78 + 0·02	E.N.E.	+3
Aberdeen	. .	29·70 − 0·07	N.E.	+7
Leith	. . .	29·67 − 0·06	N.E.	+6
Shields	. . .	29·58 − 0·15	E.N.E.	+5
Scarborough	.	29·51 − 0·23	E.	+6
Yarmouth	. .	29·39 − 0·36	E.N.E.	+8
London	. .	29·21 − 0·52	*E.N.E.	+5
Dover	. . .	29·16 − 0·55	*E.N.E.	+5
Cape Gris Nez.		29·15 − 0·57	*E.	+8
Paris	. . .	29·16 − 0·49	*S.E.	+4
Charleville	. .	29·26 − 0·40	*S.E.	3
Lyons	. . .	29·33 − 0·22	S.	
Toulon	. . .	29·47 + 0·03	S.	−5

First zone on S.W. *above trough of* S.W. *wave, anterior slope.*

Ardrossan	. .	29·70 + 0·04	E.N.E.	+7
Liverpool	. .	29·53 − 0·16	N.N.E.	+3
Portsmouth	.	29·21 − 0·50	*N.E.	+7

Second zone on S.W. *above trough of* S.W. *wave, anterior slope.*

Holyhead	. .	29·51 − 0·11	N.N.E.	+8

* The winds marked thus (*) are cyclonic.

Third zone on S.W. *above trough of* S.W. *wave, anterior slope.*

		Barom.	Direction.	Force.
Greencastle	.	29·70 + 0·17	N.E.	+ 6
Pembroke .	.	29·43 − 0·19	N.E.	+ 6
Scilly . .	.	29·30 − 0·34	N.E.	+ 6
Plymouth .	.	29·31 − 0·36	E.	+ 5
L'Orient .	.	29·21 − 0·47	E.N.E.	+ 4
Rochefort .	.	29·26 − 0·28		5

Fourth zone on S.W. *from trough of* S.W. *wave, anterior slope.*

Roche's Point.	29·52 + 0·06	N.E.	+ 5
Biarritz . .	29·37 − 0·03	S.	+ 5

Fifth zone on S.W. *from trough of* S.W. *wave, anterior slope.*

Valencia . .	29·60 + 0·01	N.N.E.	7
Corunna . .	29·40 − 0·27	S.W.	− 6

In casting the eye over the differences of fall along the line of trough, it will be seen that the greatest diminution of pressure occurred at London, Dover, and Cape Gris Nez; and on comparing the heights of the barometer at these stations (29·21 to 29·15) with the lowest point in the cyclone of the 20th in the S. of Ireland (29·46), it will be seen that not only did the cyclonic depression travel from the S. of Ireland to the N.W. of France, *but the diminution of elasticity increased as it progressed*; so that the decrease of nearly 0·60 in the S. of Ireland from a pressure of about 30·00 between April 19 and 20 appeared in the neighbourhood of Dover on the 21st, reducing the pressure there by about the same amount from about 29·71 or 29·72 the readings of the 20th. Changes of elasticity of this nature modify, but do not destroy the wave-forms, which ordinarily continue to travel in their normal directions.

The line from Valencia to Christiansund (barometer from 29·60 to 29·86) represented the crest of a slope of which the trough extended along the English Channel. This trough intersected the trough from Thurso to Paris at the stations Dover and Cape Gris Nez; and near this point of intersection a semicyclone was established. The winds of this semicyclone are marked with an asterisk (*).

The anterior slope between the crest, extending from Valencia to Christiansund and the trough along the English Channel, *did not pass onward towards the* S.E., but was apparently broken up over the British Isles by a still further reduction of pressure. It is the study of this phenomenon of decreasing (or its opposite of increasing) pressure, irrespective of the transference of masses of air either of augmented or diminished elasticity from one lo-

cality to another in certain given directions, that should engage
the closest attention of the meteorologist as being at the root of
all our atmospheric movements, and intimately connected with
the origin of cyclones and atmospheric waves. The constant
relation existing between the direction of the wind and regions
of high and low pressures so well expressed by the crests, troughs,
and slopes of atmospheric waves, is but a stepping-stone to a
still higher generalization which may be arrived at when, in
addition to the heights of the barometer and the direction and
force of wind, the temperatures at each station shall be discussed
with them. The elasticity of the air at any given moment is a
resultant of its temperature modified by the presence of aqueous
vapour; and for arriving at such higher generalization these
weather-elements, temperature and moisture, should be examined
daily during a specified period.

TABLE IV.—Distribution of Pressure and Wind over the North-
west of Europe on April 22, 1872.

First or lowest zone next above posterior trough. S.W. *wave.*

	Barom.	Direction.	Force.
Biarritz . .	29·36—0·01	S.W.	5
Rochefort . .	29·30+0·04	S.W.	5
L'Orient . .	29·13—0·08	W.S.W.	4
Scilly . . .	28·87—0·43	W.N.W.	—4
Roche's Point .	29·06—0·46	N.N.E.	+6
Valencia . .	29·25—0·35	N.N.E.	+8

This zone cuts the point of greatest depression (Scilly, 28·87),
which was probably in the trough of both waves. The S.W.
winds existed on the S.E. of this point, and the N.E. winds on
the N.W., with increasing pressures in each direction.

Second zone above the posterior trough. S.W. *wave.*

	Barom.	Direction.	Force.
Plymouth . .	28·97—0·34	S.	—4
Pembroke . .	29·01—0·42	N.N.E.	6

The trough of the N.W. wave between these stations; wind at
Plymouth changed from E. to S.

Third zone above the posterior trough. S.W. *wave.*

	Barom.	Direction.	Force.
Portsmouth .	29·15—0·06	S.S.W.	—5
Holyhead . .	29·14—0·37	E.	—3
Greencastle .	29·40—0·30	E.N.E.	6

Trough of N.W. wave between Portsmouth and Holyhead.
The E. wind at Holyhead was cyclonic around the area of least
pressure. The anterior slope of the advancing wave was charac-
terized by the greatest wind-force.

Fourth zone above posterior trough. S.W. *wave.*

	Barom.	Direction.	Force.
Toulon . .	29·63 + 0·16	S.W.	−4
Lyons . . .	29·61 + 0·28	S.E.	2
Charleville .	29·34 + 0·08	E.S.E.	+6
Paris . . .	29·31 + 0·15	S.E.	−2
Cape Gris Nez.	29·24 + 0·09	S.	−3
Dover . . .	29·22 + 0·06	S.W.	−4
London . .	29·20 − 0·01	S.	−4
Liverpool . .	29·22 − 0·31	E.S.E.	−2
Ardrossan . .	29·36 − 0·34	E.N.E.	7

Trough of N.W. wave between London and Liverpool. The S.E. winds in France were those of the posterior slope of the S.W. wave. The E.S.E. wind at Liverpool was part of the cyclonic stream of air drawing round the area of least pressure. The apparent stationary character of the barometer at London would indicate that the trough of the 21st remained in the neighbourhood during the elapsed twenty-four hours; but it is uncertain. This shows that the telegrams to the Meteorological Office should be forwarded at shorter intervals.

Fifth zone above the posterior trough. S.W. *wave.*

Brussels . .	29·32 + 0·10	S.E.	+6
Yarmouth . .	29·27 − 0·12	S.S.E.	−4
Scarborough .	29·25 − 0·26	S.	−2
Shields . . .	29·28 − 0·30	E.N.E.	+6
Leith . . .	29·42 − 0·25	N.E.	+8
Aberdeen . .	29·51 − 0·19	N.E.	+9
Nairn . . .	29·60 − 0·18	E.	+6
Wick . . .	29·68 − 0·14	E.N.E.	−4
Thurso . . .	29·67 − 0·14	N.E.	+5

Trough of N.W. wave between Yarmouth and Scarborough. The winds at Brussels, Yarmouth, and Scarborough appear to have been those of the S.W. wave, while all to the N.W. of Scarborough were those of the anterior slope of the succeeding N.W. wave.

Sixth zone above the posterior trough. S.W. *wave.*

Cuxhaven	29·33	W.	2
Oxö	29·52	N.E.	4
Skudesnaes . . .	29·56	N.N.E.	4
Christiansund . . .	29·80	Calm.	

This Table, in conjunction with the weather-maps, brings out with great distinctness the contemporaneity of no less than *four* independent (?) atmospheric arrangements. *First,* circular zones of pressure around the lowest point, extending from 28·9 to

29·3, accompanied by an approach to a cyclonic movement of wind; *second,* a stream of N.E. winds reaching the force of a gale in the E. of Scotland; *third,* a stream of S.W. winds over the N.W. of France drawn over the central parts of England into the vortical movement around the point of lowest pressure; *fourth,* the posterior slope of a S.W. wave extending from Valencia to Cuxhaven. These arrangements appear in a great measure to have been brought into existence by the diminution of pressure in the west of Ireland.

TABLE V.—Distribution of Pressure and Wind over the North-west of Europe on April 23, 1872.

First zone on S.W. *of anterior slope above trough of* S.W. *wave.*

	Barom.	Direction.	Force.
Biarritz . .	29·55+0·19	S.	−4
Rochefort . .	29·49+0·19	S.W.	5
L'Orient . .	29·28+0·15	S.W.	+8
Scilly . . .	29·04+0·17	W.S.W.	+6
Roche's Point.	29·00−0·06	N.N.E.	−5
Valencia . .	29·14−0·11	N.E.	−5

Trough of N.W. wave between Scilly and Roche's Point.

Trough of S.W. *wave.*

Plymouth . .	29·09+0·12	S.	+5
Pembroke . .	28·99−0·02	S.S.E.	+8

Trough of N.W. wave N.W. of, but near Pembroke; the barometer having fallen by the progression of the S.W. trough, the two troughs intersected near Pembroke.

First zone above trough, posterior slope. S.W. *wave.*

Portsmouth .	29·29+0·14	S.S.W.	+6
Holyhead . .	29·14	E.	3
Greencastle .	29·25−0·15	N.N.E.	+3

Trough of N.W. wave between Portsmouth and Holyhead.

Second zone above trough, posterior slope. S.W. *wave.*

Toulon . . .	29·74+0·11	S.S.W.	4
Lyons . . .	29·61	S.E.	
Charleville . .	29·55+0·21	S.W.	6
Paris . . .	29·50+0·19	S.	2
Cape Gris Nez	29·39+0·15	S.S.W.	+5
Dover . . .	29·38+0·16	S.W.	4
London . .	29·32+0·12	S.	4
Liverpool . .	29·24+0·02	E.S.E.	+3
Ardrossan . .	29·28−0·08	E.N.E.	−4

Trough of N.W. wave between London and Liverpool.

Third zone above trough, posterior slope. S.W. *wave.*

	Barom.	Direction.	Force.
Brussels	29·50 + 0·18		−4
Yarmouth	29·38 + 0·11	S.S.E.	4
Scarborough	29·32 + 0·07	S.S.E.	+3
Shields	29·32 + 0·04	E.	−3
Leith	29·31 − 0·11	E.	−4
Aberdeen	29·39 − 0·12	E.	−3
Nairn	29·39 − 0·21	E.	−2
Wick	29·46 − 0·22	E.	−4
Thurso	29·46 − 0·21	E.	−2

Trough of N.W. wave between Scarborough and Shields. Lowest barometer at Leith.

Fourth zone above trough, posterior slope. S.W. *wave.*

	Barom.	Force.
Helder	29·43	3

Fifth zone above trough, posterior slope. S.W. *wave.*

	Barom.	Direction.	Force.
Cuxhaven	29·54 + 0·21	S.E.	2
Oxö	29·56 + 0·04	N.E.	4
Skudesnaes	29·52 − 0·04	E.N.E.	−2
Christiansund	29·52 − 0·28	N.E.	+2

Trough of N.W. wave S.E. of Oxö.

A very important point to investigate in such inquiries as these, has reference to changes of elasticity in large bodies of air, such as occurred on the 20th and again on the 22nd perfectly independent of any progressive movement. Table V. shows that the distribution of pressure and wind was much the same as on the 22nd, but that during the twenty-four hours elapsed from 8 A.M. of the 22nd to 8 A.M. of the 23rd the trough of the S.W. wave had advanced but a very short distance towards the N.E. from its locality on the 22nd—also that the trough of the N.W. wave occupied nearly the same locality as it did on the 22nd, having rather receded towards the N.W. than otherwise. The most remarkable feature is that the anterior slope of the *should-be* advancing N.W. wave suffered a diminution of elasticity which resulted in a *fall* of the barometer at all stations N.W. of a line joining Cape Clear and a point between Oxö and Skudesnaes, which is exactly the *reverse* of the phenomena presented by the anterior slope of an atmospheric wave. This fall was accompanied by a *diminution of wind-force.* At all stations S.E. of the line mentioned on the posterior slope of the *should-be* receding N.W. wave, a *rising* barometer occurred, with, at some stations, an *increase of wind-*

force, again the *reverse* of the usual phenomena of a wave progressing towards the S.E. While, therefore, we had on the 23rd the area divided into two regions of rising and falling barometers, *differing from the normal conditions, the wave-form was maintained in all its integrity.* The *reversal* of the phenomena is very important (see figs. 1 & 2).

TABLE VI.—Distribution of Pressure and Wind over the Northwest of Europe on April 24, 1872.

First zone on S.W. *of anterior slope above trough of* S.W. *wave.*

	Barom.	Direction.	Force.
Corunna . .	29·77	S.W.	6
Valencia . .	29·31 + 0·17	W.	−4

Trough of S.W. *wave.*

| Biarritz . . | 29·81 + 0·26 | S. | −3 |
| Roche's Point . | 29·29 + 0·29 | W.S.W. | −3 |

First zone above trough, posterior slope. S.W. *wave.*

Rochefort . .	29·73 + 0·24	W.	5
L'Orient . .	29·61 + 0·33	W.	−5
Scilly . . .	29·40 + 0·36	W.S.W.	−5

Second zone above trough, posterior slope. S.W. *wave.*

Plymouth . .	29·42 + 0·33	S.S.W.	−2
Pembroke . .	29·34 + 0·35	S.W.	−3
Holyhead . .	29·30 + 0·16	S.S.E.	+4
Greencastle .	29·25	E.S.E.	+5

Third zone above trough, posterior slope. S.W. *wave.*

Portsmouth .	29·53 + 0·24	S.W.	6
Liverpool . .	29·42 + 0·18	S.S.E.	3
Ardrossan . .	29·35 + 0·07	S.S.E.	−3

Fourth zone above trough, posterior slope. S.W. *wave.*

Toulon . . .	29·84 + 0·10	S.S.W.	−1
Lyons			
Charleville .	29·74 + 0·19	S.W.	6
Paris . . .	29·72 + 0·22	S.	2
Cape Gris Nez	29·59 + 0·20	S.W.	5
Dover . . .	29·58 + 0·20	S.W.	+5
London . .	29·52 + 0·20	S.S.W.	−3

Fifth zone above trough, posterior slope. S.W. *wave.*

	Barom.	Direction.	Force.
Brussels	29·68 + 0·18	S.W.	−3
Yarmouth	29·54 + 0·16	S.S.E.	−3
Scarborough	29·45 + 0·13	S.W.	3
Shields	29·42 + 0·10	S.S.W.	3
Leith	29·40 + 0·09	E.	−3

Sixth zone above trough, posterior slope. S.W. *wave.*

	Barom.	Direction.	Force.
Helder	29·61 + 0·18	S.W.	+5
Aberdeen	29·44 + 0·05	S.E.	3
Nairn	29·43 + 0·04	E.S.E.	2
Wick	29·50 + 0·04	S.E.	4
Thurso	29·47 + 0·01	E.S.E.	2

Seventh zone above trough, posterior slope. S.W. *wave.*

	Barom.	Direction.	Force.
Cuxhaven	29·67 + 0·13	S.	3
Oxö	29·67 + 0·11	E.N.E.	4
Skudesnaes	29·62 + 0·10	S.E.	+4
Christiansund	29·61 + 0·09	Calm	

This Table is one of the most interesting of the series; it exhibits a general rise of the barometer over the entire area, and the establishment of the posterior slope of the N.W. wave, the direction of the trough of which has been specified in Tables III., IV., and V. It would appear at first sight that the wave had progressed *towards the* N.W.; but it is likely that this appearance arose from the irregularity of increase of elasticity being greatest along the English Channel. It is, however, noteworthy that with the extension of the trough towards the N.W., or in other words, of zones of decreasing pressure from the crest towards the N.W., the winds in the N.W. should be those of the S.W. wave, the zones of which are given in the Table. Space prevents the further discussion of the barometric curves and sections, which are capable of yielding a valuable harvest of results; but as the Tables contain the heights of the barometer and their differences for each day, the reader can easily construct them for himself; indeed this course is recommended for the elucidation of the preceding Tables.

In the year 1843 the late Sir John Herschel, writing on the subject of barometric fluctuations, said "it would be no small meteorological discovery if by the study of the characters and progress of barometrical fluctuations we could either make out any law of the greater ones which would enable us even roughly to predict them, or any peculiarity in their physiognomy by which we could recognize them in their earlier stages, as by this we

might possibly be led to the prediction of great storms." *Although up to this time no such law has been* RECOGNIZED *by our leading meteorologists,* it may be permissible to inquire if the steps which have hitherto been taken, such as are exhibited in the preceding Tables, and capable of being rendered still more intelligible by curves and sections, have not developed these principles, viz. that the vast bodies of air, possessing regular gradations of pressure analogous to *wave-forms* associated with certain definite winds which *always* accompany these gradations of pressure, move in certain directions, viz. from N.W. to S.E., and from S.W. to N.E., and also if such regular motions are not of the nature of "laws" different from Buys Ballot's law. The present paper shows that other forces of a still higher character than wave-motion come into operation, producing changes of elasticity the laws of which require to be investigated. This work is a laborious one, but without it little progress will be made in meteorology. The study of atmospheric waves must conduce to a clearer conception of the changes giving rise to the production of new or the modification of existing waves.

XVIII. *Notices respecting New Books.*

British Rainfall, 1871. *By* G. J. SYMONS. London: Stanford, Charing Cross.

THIS annual, which has made its appearance recently, contains the usual amount of information on the rainfall during the past year in the British Isles, accompanied with a few most interesting notices of rainfall on the Continent and in India. We notice it especially on account of some important experiments on rainfall at different elevations, particularly at Aldershot Camp, by Colour-Serjeant Arnold, who found that at heights of 6 feet and 30 feet respectively gauges with the receiving apertures tilted at an angle of 45° and kept to the wind by powerful vanes, collected as nearly as possible the same amount of rain; while gauges at 6 feet and 25 feet of elevation with the receiving apertures placed horizontally collected different amounts, the greatest quantity being found in the lowest gauge. A collateral phenomenon, although not mentioned in the volume, may yet be gathered from the Tables inserted: it is that during four years (1868 to 1871) at Rotherham a gauge placed horizontally 5 feet above the ground gathered 71 per cent. of the rain gathered in a rotating gauge (angle 45°) at the same elevation, the quantity gathered at Aldershot in the horizontal gauge as compared with the tilted gauge at an elevation of 6 feet being 70 per cent. These results at two stations, extending over four and three years respectively, show that we have much to learn as regards rainfall, especially in relation to the *standard* quantity; for if gauges separated from each other by a vertical space of 24 feet gather the same quantity, our

results must be influenced by the smaller quantity gathered in our horizontal gauges in proportion to the differences. We hope Mr. Symons will give especial attention to this subject. We hear much now-a-days of the need for Government aid to science ; and yet our Government establishments have not detected these anomalies !

XIX. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 75.]

April 11, 1872.—The Earl of Rosse, D.C.L., Vice-President, in the Chair.

THE following communication was read :—
    "The Action of Oxygen on Copper Nitrate in a state of Tension." By J. H. Gladstone, Ph.D., F.R.S., and Alfred Tribe, F.C.S.

In our experiments on the action between copper and nitrate of silver in solution, we frequently noticed that the tips of the silver crystals became red, as though coated with a thin layer of metallic copper.

This apparent deposition of a positive on a more negative metal of course raised our curiosity, and led us to look closely into the circumstances under which it occurred. We found that it took place only when the nitrate of silver was exhausted, and only on those silver crystals which remained in metallic connexion with the copper. We found, too, that the cupreous coating formed most readily where air had the freest access, and, in fact, that it would not form at all in vessels from which oxygen was excluded, nor on those white crystals which were far below the surface of the liquid, though they might be in immediate contact with the copper plate. When an inverted jar was filled with nitrate-of-copper solution and silver crystals resting on branches of copper, and the liquid was displaced by oxygen gas, it was found that the tips of the crystals became red, and the solution gradually filled the jar again by the absorption of the gas. In the same way the oxygen was absorbed from air, or from its mixtures with hydrogen or carbonic anhydride.

This action was further studied by employing plates of the two metals instead of copper covered with silver crystals. When the two plates, connected by a wire, were partially immersed in an ordinary aqueous solution of copper nitrate, it was found that a slight yellowish deposit made its appearance speedily all over the silver plate, and went on increasing for a day or two, while at the air-line there was a thicker deposit, which gradually grew and extended itself a little below the surface. This deposit changed from yellowish to red, and under the microscope presented a distinctly crystalline appearance.

Thinking that this slight crust all over the silver plate was due to air dissolved in the solution itself, we took advantage of the re-

action to prepare copper nitrate absolutely free from dissolved oxygen. An ordinary solution of the salt mixed with some silver nitrate was placed in a narrow cylinder, with a long piece of copper-foil arranged somewhat spirally, so as to retain the deposited silver on its surface, and allowed to rest for twenty-four hours. The solution thus obtained was exposed to the action of the conjoined copper and silver plates ; but even after some hours there was no dimming of the lustre of the silver plate, except at the air-line, which was sharply defined.   The same solution, shaken for some time in the air, produced a yellowish deposit on the white metal in three minutes.

The colour and general appearance of this crust, together with its formation only where oxygen can be absorbed, showed that it was not metallic copper, but the suboxide.   This was further proved by the action of dilute sulphuric acid, which resolves it at once into red metallic copper and copper sulphate.   There is also another curious reaction, which can only be properly observed under a microscope.   When treated with a solution of silver nitrate, this cupreous deposit does not give the ordinary crystals of the white metal; in fact it is only slowly acted upon ; but presently there shoot forth thin threads of silver, which run through the liquid, often twisting at sharp angles, while the yellowish crystals change to black.   This also was found to be a property of the suboxide of copper.

This deposition of oxide on the silver is accompanied by a corresponding solution of copper from the other plate.   Thus, in an experiment made with nitrate-of-copper solution that had been exposed to air, and which was allowed to continue for four days, there was found :—

<blockquote>
Gain of silver plate   0·016 grm.
Loss of copper plate 0·015 grm.
</blockquote>

The copper necessary for the production of 0·016 grm. of suboxide would be a little above 0·014 grm.

The wire connecting the two plates in this experiment is capable of deflecting a galvanometer.   The current takes place through the fluid from copper to silver—that is, in the same direction as if the copper had been dissolved by an acid and hydrogen evolved on the silver plate.

If the two plates have their sides parallel, the suboxide is deposited not merely on that side of the silver plate which faces the copper, but after about a minute on the other side also, showing that in this, as in other cases, the lines of force curve round.

It became interesting to consider what started this electric current. The original observations convinced us that it was not due to the action of oxygen on the copper ; but, to make the matter more certain, bright copper and silver plates in conjunction were immersed, the copper in a pure, *i. e.* deoxygenized, solution of nitrate of copper, the silver in an oxygenized solution : the two liquids communicated through the diaphragm of a divided cell.   In half an hour the silver plate was covered with a reddish film, while not a trace of tarnish

was perceptible on the copper. On continuing this experiment for three hours, it was found that the copper plate lost 0·003 grm., and the silver plate was increased by 0·004 grm. On cleaning the plates, and reversing their position, the copper was covered with a film of oxide, while the silver remained free from cupreous deposit. We believe therefore that, through the simultaneous action of the two metals, the dissolved salt is put into such a state of tension that oxygen brings about a chemical change which otherwise would be impossible, and that this change is initiated in close proximity to the more negative metal.

Though we have examined only this particular reaction, we have satisfied ourselves that it is not an isolated fact. Each of the elements concerned may be replaced by others: thus the sulphate may be substituted for the nitrate of copper, or platinum may be used instead of silver; chlorine may take the place of oxygen, with the production of the subchloride instead of the suboxide; and zinc may be employed as the positive metal, with zinc chloride as the salt in solution, in which case copper may be taken as the negative metal, and on its surface will form a deposit of oxide of zinc.

April 25.—George Biddell Airy, C.B., President, in the Chair.

The following communication was read :—

" On a supposed Periodicity in the elements of Terrestrial Magnetism, with a period of $26\frac{1}{3}$ days." By George Biddell Airy, Astronomer Royal.

In a paper published in the 'Proceedings of the Imperial Academy of Sciences of Vienna,' vol. lxiv., Dr. Karl Hornstein has exhibited the results of a series of observations which appeared to show that the earth's magnetism undergoes a periodical change in successive periods of $26\frac{1}{3}$ days, which might with great plausibility be referred to the rotation of the sun.

It appeared to me that the deductions from the magnetic observations made at the Royal Observatory of Greenwich, and which are printed annually in the 'Greenwich Observations,' or in the detached copies of ' Results of Magnetical and Meteorological Observations made at the Royal Observatory of Greenwich,' would afford good materials for testing the accuracy of this law, as applicable to a series of years. The mean results of the measured hourly ordinates of the terrestrial magnetic elements are given for every day ; and it is certain that there has been no change of adjustments of the declination and horizontal-force instruments in the course of each year. For the horizontal-force instrument the temperature of the room has been maintained in a generally equable state, and in later years it has been remarkably uniform.

It is easy to see that an error of a single day, or of a large fraction of a day, in the beginning of each period, is of no importance, provided that the errors are not permitted to accumulate. It was allowable, therefore, to take successive periods of 26, 26, 27, 26, 26, 27, &c. days ; and in instances when a single day was omitted, or even

two days, no sensible error would be introduced by interpolating between the numbers for the days immediately preceding and following the omitted days.

The years selected for this examination were 1850, 1851, 1852, 1868, 1869, 1870; and the beginning of the first period in each year after the first was thus found :—Fourteen periods of $26\frac{1}{3}$ days each amount to $368\frac{2}{3}$ days. For convenience, after completion of the annual winter adjustments, the first period in 1850 was made to commence on January 17; therefore the first period in 1851 was commenced on January 21, and that in 1852 on January 25. Similarly, the first periods in 1868, 1869, 1870 commenced on January 1, 4, and 8 respectively; and the beginnings in the three later years are not unconnected with those in the three earlier years: for, from 1852, January 25, to 1868, January 1, are 5820 days, and 221 periods of $26\frac{1}{3}$ days each are $5819\frac{2}{3}$ days; but as the years are widely separated, and a small error of period would produce a large discordance, it has appeared best to exhibit the results of the two three-years' groups separately.

Some periods, in which there were unusually large interruptions, or which were partly occupied with experiments, were omitted entirely. The following is a complete list of periods omitted :—In 1850, that beginning with December 24 for horizontal force; in 1851, that beginning with March 14 for western declination, and those beginning with March 14, June 28, July 24, for horizontal force; in 1852, those beginning with February 20, May 9, December 6, for both elements ; in 1868, those beginning with February 23 for declination, and January 1, January 27, February 23, and December 8 for horizontal force; in 1869, those beginning with October 21 and December 12 for both elements; and in 1870, those beginning with June 15 and December 16 for declination, and that beginning with December 16 for horizontal force. Interpolations of three days occur only in the following instances :—1850, (dec.) Feb. 4–6, (h. f.) Feb. 9–11, July 23–25; 1851, (dec.) Feb. 18–20, Oct. 20–22, (h. f.) June 9–11; 1852, (dec.) Feb. 7–9; 1868, (dec.) Feb. 15–17, (h. f.) none; 1869, (dec.) June 6–8, (h. f.) June 6–8; 1870, (dec.) Sept. 24–26, (h. f.) Sept. 24–26.

The mean values of each element for each progressive day in every period of the several years, uncorrected for the proportional part of secular change through the 26 days, and omitting the imperfect 27th day, are as follows :—

Progressive day of each period.

Declination (omitting 22° in 1850, 1851, 1852; 20° in 1868, 1869; 19° in 1870).

Year.	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26
1850.	23·62	22·91	22·96	23·23	23·05	23·13	23·07	23·00	22·73	23·64	23·39	23·56	23·19	22·83	23·29	23·06	22·75	22·54	23·03	23·15	23·21	23·32	23·24	23·09	23·47	23·17
1851.	17·95	18·25	18·26	18·18	17·92	18·03	17·91	17·90	18·32	18·32	18·48	19·25	19·22	18·79	18·37	18·35	18·38	18·17	18·11	18·45	18·64	18·59	18·36	18·49	18·53	18·63
1852.	17·93	17·88	18·27	17·93	17·47	17·70	17·66	17·36	17·49	17·30	16·86	16·82	16·92	17·16	17·28	16·99	17·59	17·38	17·18	17·11	17·39	17·01	17·23	16·90	16·97	17·20
1868.	13·85	13·16	12·83	13·2	12·90	13·09	13·42	13·27	13·17	12·57	12·47	12·59	12·50	12·84	12·99	12·75	12·75	12·77	12·66	12·67	12·78	13·05	13·11	13·05	12·95	13·34
1869.	5·53	5·34	5·16	5·10	4·88	4·96	4·93	4·40	4·78	4·60	5·02	4·50	4·74	4·78	4·62	4·87	4·53	4·47	4·39	4·84	4·57	4·54	4·59	4·50	4·62	4·69
1870.	53·41	53·28	53·03	53·53	53·13	53·27	54·05	53·62	53·54	53·42	53·21	53·27	53·36	53·22	53·37	53·29	53·18	53·25	52·73	52·98	53·09	52·91	52·78	52·33	52·65	52·51

Horizontal Force (omitting ·11000 in 1850, 1851, 1852, and ·14000 in 1868, 1869, 1870).

Year.	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26
1850.	310	288	242	269	262	278	259	293	292	280	252	250	208	250	256	245	272	236	234	224	221	251	235	243	236	256
1851.	582	557	545	538	561	544	530	555	530	538	538	565	598	586	568	578	565	553	547	522	530	556	545	539	580	559
1852.	381	357	332	325	348	319	345	350	321	386	404	410	388	353	397	388	412	364	356	357	364	353	352	342	357	373
1868.	283	284	299	307	290	275	265	258	222	265	278	248	249	247	280	305	283	298	303	298	300	298	293	314	321	317
1869.	299	302	284	273	267	270	291	281	295	267	297	287	304	307	284	277	281	313	286	277	273	296	275	243	258	247
1870.	856	853	848	861	888	885	906	930	920	918	941	942	967	955	966	943	938	922	925	905	908	902	882	886	885	892

Taking the means of these numbers separately for the groups of years 1850–1852 and 1868–1870, and applying the proportional parts of secular correction, at the rate of $+0·62$ for 26 days in western declination, and at the rate of $-·00013$ for 26 days in horizontal force, we have the following results:—

Range of years.	Progressive day of each period.																									
	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26
Western Declination (omitting 22° 10' in the first range of years, and 20° in the second).																										
1850–1852.	9·83	9·70	9·88	9·85	9·58	9·74	9·70	9·59	9·71	9·97	9·83	10·15	10·07	9·91	9·99	9·84	9·96	9·78	9·88	10·04	10·24	10·16	10·15	10·06	10·25	10·29
1868–1870.	4·10	3·95	3·72	3·95	3·74	3·89	4·28	3·93	4·03	3·75	3·82	3·72	3·82	3·93	4·00	4·04	3·88	3·92	3·70	3·97	3·97	4·02	4·03	3·86	4·00	4·13
Horizontal Force (omitting ·11300 in the first range of years, and ·14400 in the second).																										
1850–1852.	124	101	72	75	88	77	75	95	77	93	93	102	92	89	99	96	107	75	69	58	61	76	65	68	78	83
1868–1870.	79	83	76	78	80	74	84	86	75	78	100	86	101	96	102	100	92	102	95	83	83	88	71	69	75	72

The mean for declination in 1870 and, still more remarkably, the mean for horizontal force in 1870 appear to exhibit an increase about the 14th day. But I do not remark in the other means, either as given in numerals or as projected in curves, any thing to support the idea of an inequality periodical in the 26⅓ days. It might almost be suspected that the secular changes used in the period 1850–1852 are too large; but no alteration of these renders the inequality of 26⅓ days more probable.

Dr. Hornstein's investigation was limited to observations made in 1870.

As the diurnal changes of the magnetic elements appear to be due to the action of the sun, it seemed probable that any irregularity in that action depending on the face of the sun presented to the earth might be principally conspicuous in the diurnal change. I therefore thought it prudent to avail myself of the computations of diurnal change of western declination already prepared at the Royal Observatory. The following numbers have been computed in exactly the same manner as those for absolute western declination.

Total diurnal change of western declination.

Year or range of years.	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15	16	17	18	19	20	21	22	23	24	25	26
											Progressive day of each period.															
1850.	12·16	13·01	14·14	12·69	11·29	11·94	13·60	10·82	12·21	13·33	11·99	12·63	12·81	12·84	12·55	13·11	13·06	13·23	14·04	14·13	14·29	11·64	12·46	12·35	12·73	14·29
1851.	11·83	10·58	10·96	11·21	11·45	13·75	10·99	13·14	12·82	12·31	11·23	11·92	12·92	13·15	11·04	13·27	11·62	10·41	11·31	11·77	10·41	11·02	11·54	10·25	11·65	12·19
1852.	12·25	14·95	14·05	14·52	12·08	11·66	13·15	14·35	15·73	12·82	14·16	13·47	13·82	14·43	13·45	15·15	12·43	14·59	14·68	13·27	13·55	13·79	13·18	13·35	13·35	12·73
1850–1852.	12·08	12·85	13·25	12·81	11·61	12·45	12·58	12·60	13·59	12·82	12·43	12·67	13·18	13·47	12·35	13·84	12·37	12·74	13·34	13·06	12·75	12·15	12·49	11·88	12·59	13·07
1868.	11·39	10·68	11·89	12·14	11·86	13·45	12·58	12·78	13·95	11·45	13·47	15·36	13·75	11·09	10·05	11·01	10·30	10·62	10·25	10·35	9·70	10·28	11·00	11·02	12·22	11·58
1869.	11·46	11·61	11·55	13·58	12·93	14·49	12·97	13·09	13·30	14·32	13·78	14·03	14·81	14·18	14·03	14·28	13·09	13·93	14·28	14·13	12·68	11·72	12·68	12·42	13·33	12·53
1870.	16·01	14·93	15·24	15·23	13·88	15·19	15·48	16·09	15·31	14·91	14·03	13·70	15·02	14·49	13·07	14·70	14·27	13·48	16·25	15·40	15·99	15·87	16·06	15·12	16·43	15·39
1868–1870.	12·95	12·41	12·89	13·65	12·89	14·38	13·64	13·99	14·19	13·56	13·76	14·36	14·53	13·25	12·38	13·33	12·55	12·63	13·59	13·29	12·79	12·62	13·25	12·85	13·99	13·17

In 1870, the diurnal change appears to be less from the 10th to the 18th day than in other parts of the period. In 1869 it appears to be larger at the same part of the period. In 1868, the small values occur principally towards the end of the period.

[Continued from vol. xliii. p. 544.]

February 7, 1872.—Joseph Prestwich, Esq., F.R.S., President,
in the Chair.

The following communications were read :—

1. "Further Notes on the Geology of the neighbourhood of
Malaga." By M. D. M. d'Orueta. Communicated by the President.

In this paper, which is a continuation of a former note laid
before the Society (see Q. J. G. S. xxvii. p. 109), the author com-
menced by stating that his former opinion as to the Jurassic age of
the rocks of Antequera is fully borne out by later researches upon
their fossils. They apparently belong to the Portlandian series.
The author made considerable additions to his description of the
Torcal, near the foot of which he has found a sandstone containing
abundance of *Gryphæa virgula* and *Ostrea deltoidea*. This he re-
gards as equivalent to the Kimmeridge Clay. In the Torcal he has
also found a soft, white, calcareous deposit, overlying the limestones of
supposed Portlandian age, and containing a fossil which he identifies
with the Tithonian *Terebratula diphya*. The author discussed the
peculiar forms assumed by the rocks of the Torcal under denuda-
tion, which he supposed to be due originally to the upheaval caused
by the rising of a great mass of greenstone, portions of which are
visible at the surface on both sides of the range.

2. "On the River-courses of England and Wales." By Prof. A.
C. Ramsay, LL.D., F.R.S., F.G.S.

The author commenced by describing the changes in the physical
conformation of Britain during the Jurassic and Cretaceous periods,
and the relations which the deposits found during those periods bore
to the Palæozoic rocks of Wales and the north-west of England.
He stated that the Miocene period of Europe was essentially a con-
tinental one, and that it was closed by important disturbances of
strata in Central Europe, one effect of which would be to give the
Secondary formations of France and Britain a slight tilt towards
the north-west. To this he ascribed the north-westerly direction
of many of the rivers of France ; and he surmised that at this
period the rivers of the middle and south of England also took a
westerly course. The westerly slope of the Cretaceous strata of
England was also, he considered, the cause of the southern flow
of the Severn, between the hilly land of Wales and the long slope
of chalk rising towards the east. The Severn would thus establish
the commencement of the escarpment of the Chalk, which has since
receded far eastward.

The author believed that after the Severn had cut out its valley
the Cretaceous and other strata were gradually tilted eastwards,
causing the easterly course of the Thames and other rivers of
southern and eastern England. In these and other cases adduced
by the author, the sources of these rivers were originally upon the
Chalk near its escarpment ; and it is by the recession of the latter

(which was followed by the formation of the oolitic escarpment) that its present relation to the river-courses has been brought about. The author also referred to the courses followed by the rivers of the more northern part of England, and indicated their relations to the general dip of the strata.

March 6, 1872.—Prof. Duncan, F.R.S., Vice-President, in the Chair.

The following communications were read:—

1. " *Prognathodus Güntheri* (Egerton), a new Genus of Fossil Fish from the Lias of Lyme Regis." By Sir P. de M. Grey Egerton, Bart., M.P., F.R.S., F.G.S.

In this paper the author described a new form of fossil fish having a broad premaxillary plate somewhat resembling the incisor tooth of a gigantic Rodent, a single maxillary plate like that of *Callorhynchus*, and a mandibular dental apparatus closely resembling that of *Cochliodus*. For this form he proposed the establishment of the new genus *Prognathodus*, and named the species *P. Güntheri*. *Ischyodus Johnsoni*, Agassiz, also probably belongs to this genus, as it agrees with *P. Güntheri* in the characters of the premaxillary teeth. The author was doubtful as to the exact position of this genus, which had a head extended in a horizontal instead of a vertical plane, suggesting a resemblance to *Zygæna*, but covered with hard plates like the head of a sturgeon, and exhibited in the dental apparatus the curious combination indicated above.

Dr. Günther pointed out the interest attaching to the dentition of this fossil fish as being an additional evidence in favour of the connexion between the Ganoid and Chimæroid forms. The existence of three teeth instead of one on each side of the jaw, as in *Ceratodus* and others, presented in it a generic character; but the type was still the same. On one point he slightly differed from the view of the author; and that was as to the application of the terms maxillary and premaxillary to the teeth. He thought the former belonged rather to the pterygo-palatine arch, and that the teeth in the front of the jaw should be regarded as vomerine. He illustrated this by reference to the jaws and dentition of sharks, Chimæroids, and certain Ganoids, such as sturgeons. In these the teeth, instead of being connected with the maxillary and premaxillary bones, were, in fact, connected with the pterygo-palatine arch. He considered that this furnished additional grounds for including all three forms in one subclass.

2. " On two specimens of *Ischyodus*, from the Lias of Lyme Regis." By Sir P. de M. Grey-Egerton, Bart., M.P., F.R.S., F.G.S.

In this paper the author noticed a new example of the greatly developed rostrum of a male Chimæroid, an inch shorter, more slender, and more attenuated at the apex than that of *Ischyodus orthorhinus*, Egerton, having a projecting median rib along the upper surface, and the tubercles of the lower part smaller and fewer than in *I. orthorhinus*. For this form the author proposed the name of

I. leptorhinus. Also a dorsal fin-spine, with the cartilages to which it was articulated, showing the mechanism of its attachment very clearly. This spine differs from that of *I. orthorhinus* in being straighter and smoother, and having fewer and smaller tubercles. The author regarded it as probably belonging to *I. leptorhinus.*

3. " How the Parallel Roads of Glen Roy were formed." By Prof. James Nicol, F.G.S.

In this paper the author endeavoured to explain, in accordance with the marine theory of the origin of the Parallel Roads of Glen Roy, the coincidence of the level of these terraces with that of the different cols, and also how the same sea could have produced terraces at different levels in different valleys. He assumed that, during the gradual elevation of the land, the gradual closing of the straits between its separate masses by the elevation of the cols above the surface would, by checking the eastward flow of the tidal current, cause the sea-level in the western bays to remain stationary relatively to the rising land; and during this period the marine erosion would take place along a line corresponding in level to the col. Hence, in Glen Gloy, which has only one col, the highest in the system, the highest road only was formed; and Glen Gloy remained unaffected by the stoppage of those cols which produced three roads at lower levels in Glen Roy, the lowest of them also extending round Glen Spean.

March 20, 1872.—Prof. John Morris, Vice-President, in the Chair.

The following communication was read:—

" On the Wealden as a Fluvio-lacustrine Formation, and on the relation of the so-called ' Punfield Formation ' to the Wealden and Neocomian." By C. J. A. Meÿer, Esq., F.G.S.

In this paper the author questioned the correctness of assigning the Wealden beds of the south-east of England to the delta of a single river; he considered it more probable that they are a fluvio-lacustrine rather than a fluvio-marine deposit, and attributed their accumulation to the combined action of several rivers flowing into a wide but shallow lake or inland sea. The evidence adduced in favour of these views was mainly as follows :—The quiet deposition of most of the sedimentary strata, the almost total absence of shingle, the prevalence of such species of Mollusca as delight in nearly quiet waters, the comparative absence of broken shells such as usually abound in tidal rivers, and the total absence of drift wood perforated by Mollusca in either the Purbeck or Wealden strata.

This Wealden lacustrine area the author supposed to have originated in the slow and comparatively local subsidence of a portion of a land-surface just previously elevated. He considered that during the Purbeck and later portion of the Wealden era the waters of such lacustrine area had no direct communication with the ocean. The changes from freshwater to purely marine conditions, which are twice apparent in the Purbeck beds, and the final change from Wealden to Neocomian conditions at the close of the Wealden, were

attributed to the sudden intrusion of oceanic waters into an area below sea-level.

The author then pointed to the traces of terrestrial vegetation in the Lower Greensand as evidence of the continuance of river-action after the close of the Wealden period.

In the concluding portion of his paper the author refered to the relation of the Punfield beds of Mr. Judd to the Neocomian and Wealden strata of the south-east of England. From the sequence of the strata, no less than on palæontological evidence, he considered the whole of the so-called " Punfield formation " of the Isle of Purbeck to be referable to the Lower Greensand of the Atherfield section.

XX. *Intelligence and Miscellaneous Articles.*

RESEARCHES ON THE ELECTRIC JET IN RAREFIED GASES, AND IN PARTICULAR ON ITS MECHANICAL FORCE. BY MM. A. DE LA RIVE AND E. SARRASIN.

ONE of us has already occupied himself with the phenomenon of the rotation experienced, under the magnetic influence, by the electrical jet produced, by means of the Ruhmkorff coil, in a rarefied gas. We have just resumed the experimental study of this phenomenon, in order to effect a better determination of its nature and its conditions.

In this extract we limit ourselves to a statement of only a few of the results we have obtained, reserving the details of the experiments, and the description of the apparatus, for the memoir which is about to be printed.

In a first series of experiments we sought to determine the influence of the degree of rarefaction of the gas, and of its nature, on the velocity of rotation of the jet. For this purpose we placed on each of the poles of a powerful electromagnet a cylindrical glass jar, in which the electric jet, passing from a knob in the centre to a ring, could, under the magnetic influence, turn in a horizontal plane like the hand of a watch. The two jars were perfectly similar; the magnetic force of the two poles on which they rested was exactly the same; and the same current traversed successively the rarefied gas in the two jars arranged one after the other in the circuit. The result was that when the two gaseous media were identical, the velocity of rotation of the jets was the same in both jars; this we verified by numerous experiments. That velocity, then, could only vary with the condition or the nature of the gaseous media, since all the other circumstances were unchanged.

Thus, with atmospheric air, the number of turns of the jet in 30 seconds, at 13 millims. pressure, was 105, alike in both jars; now, the air in one of the jars being kept at 13 millims. pressure while the pressure was increased to 26 millims. in the other, we had only 63 turns in the latter, but 102 turns in the former; and at 39 millims. pressure the number of turns was only 54, while it was 96 in

the jar of which the air had retained the pressure of 13 millims. The small differences of velocity observed in the jar in which the pressure remained the same were occasioned by this—that the augmentation of pressure in the other, of course, diminished somewhat the intensity of the current transmitted successively through the two jars.

This experiment (repeated several times, and always with the same result, though under different forms) shows that the velocity of rotation of the jet varies, *cæteris paribus*, with the density of the gaseous medium, but that it diminishes in a less ratio than that in which the density increases.

Carbonic acid gave the same result as atmospheric air. The gas being at 30 millims. pressure in one of the jars, and at 15 millims. in the other, the number of turns of the jet in 30 seconds was 30 in the first, and 50 in the second.

One of the jars being filled with air, and the other with carbonic acid, and the two gases being at the same pressure of 20 millims., the number of turns in 30 seconds was 40 in the air and only 30 in the carbonic acid—which shows that the density of the gas, independently of the pressure, exerts a great influence on the velocity of rotation.

We likewise experimented on some other gases; but, with the exception of carbonic acid, the compound gases, being rapidly decomposed by the electric jet, cannot give exact results. Hydrogen is not in the same category; but in it the velocity of rotation is so great that it is very difficult to appreciate it directly. We shall afterwards return to the results given by the employment of this gas.

These first experiments having shown the resistance opposed to the electric jet by the medium in which it moves, we were led to try what resistance would be exerted upon it by a moveable solid obstacle.

With this view we suspended in a wide and high bell-glass, by means of a cocoon-thread, a small square of gummed paper disposed so as to present its vertical face to the action of the horizontal electric jet. Every time that the jet, in its rotation under the action of the magnetism, encountered the square of paper, this received an impulse, renewed at each passage of the jet, so that at last it oscillated like a pendulum. The air in which the experiment took place was at from 15 to 20 millims. pressure.

For a better study of this mechanical action of the jet, we replaced the little pendulum by an ivory swivel with an agate socket, moveable on a pivot placed in the centre, so that it could revolve in a horizontal plane, parallel with the jet, but a little below it. Each of the two extremities of the ivory needle carried a vertical disk of light glass 5 centims. in diameter, which the electric jet in its rotation touched in passing. Thence the swivel received an impulsion which ended in impressing on it a movement of rotation, the velocity of which went on for a long time increasing, and only became constant when the resistance of the surrounding rarefied air and the friction on the pivot balanced the accelerating force.

We shall here cite only two experiments, which give the number of turns of the swivel in 30 seconds in atmospheric air, and in hydrogen, at various degrees of pressure.

In Air.		*In Hydrogen.*	
Pressure. millims.	Number of turns.	Pressure. millims.	Number of turns.
10	11	38	32
8	16	18	46
5	25	1	54
4	30		

In the last experiment the jet was no longer visible; there was only a slightly luminous circular sheet between the central knob and the metallic ring, which, turning rapidly under the magnetic action, carried the swivel with it.

A numerous series of experiments was then devoted to the study of the variations in intensity to which the current producing the jet is subject when the latter turns the swivel. To measure these variations, we employed the same derivation-apparatus which we used in our preceding work, and which M. de la Rive has described in his first memoir on the subject.

We thus ascertained that the intensity of the current sensibly diminishes when the jet sets the swivel going. In air at 9 millims. pressure the motion given to the swivel by the jet induced a diminution of 10° in the deflection of the galvanometer, viz. from 42° to 32°, and in air at 8 millims. a diminution of 11°, viz. from 45° to 34°. In another experiment, with the air at 14 millims. pressure, the swivel making 18 turns in 30 seconds, and the jet 72 turns, the deflection fell 8°, or from 32° to 24°.

With hydrogen the decrease of intensity is less sensible than with air; this results from various causes—in particular, from the greater electric conductivity of that gas. In the most favourable conditions it is scarcely more than 5°; and with high degrees of rarefaction it is still less.

From the diminution of intensity produced by the resistance of the swivel, we presumed that, without the latter, the resistance which the surrounding gaseous medium must oppose to the jet in its rotation would by itself produce the same effect, although to a less degree. This, in fact, was confirmed by experiment. Thus in the large bell containing air at 19 millims. pressure, we obtained a diminution of 4° in the deflection of the galvanometer, it having fallen from 43° to 39°. The velocity of rotation of the jet was 45 turns in 30 seconds. On reducing the pressure to 8 millims., the velocity of the jet being 87 turns in the same time, we had equally a diminution of intensity of 4°. Here the increase in the velocity of the jet (which was about double) compensated the diminution in the mass of the gas (which was about three fifths less).

In determining the diminution of intensity of the current with and without the employment of the swivel, we obtained with atmospheric air a diminution of 8° (from 28° to 20°) at 18 millims. pressure,

with a velocity of 102 turns of the jet in 30 seconds—and on put-
ting-in the swivel, a diminution of $10°$ for a velocity of 22 turns of
the swivel and 82 turns of the jet in 30 seconds.

With hydrogen, without the swivel the rotation induces no sen-
sible diminution of intensity.   It must be remarked that in this case
the jet disappears and divides into an infinite number of threads
throughout the mass of the gas, as an ordinary electric current in a
liquid conductor does in the same circumstances, so that the whole
of the gaseous sheet turns under the action of the magnet.

Nevertheless, before concluding from these last experiments that
the diminution of the intensity of the current is due to the mecha-
nical force exerted by the jet upon either the swivel or the gaseous
medium, we must take account of a circumstance which may have
some influence on that diminution, viz. the cooling which the jet
undergoes in its rotation by contact with both the rotating disk and
the gaseous medium.   In these last instances the cooling can be as-
certained by the manometer, which indicates a slight increase of
pressure when the jet is rotating ; and this can only be occasioned
by its coming into contact in its rotation with different parts of the
medium and heating it so much the more.   Yet, if we compare this
effect with the diminution of intensity of the current, we do not find
a sufficient ratio to account for that influence. ̄ Thus, on employing
a smaller bell-glass with air under a pressure of 10 millims., we ob-
tain, for a velocity of 90 turns in 30 seconds, $8°$ diminution of inten-
sity of the current (from $42°$ to $34°$), while the increase of pres-
sure is only ·08 of a millimetre—indicating a very slight elevation
of the temperature of the gaseous medium, and consequently a very
slight cooling of the jet.

But more than that.   If, by means of apparatus arranged for the
purpose, we give to the bell containing the rarefied gas and the
swivel a movement of rotation on its axis, there is almost no dimi-
nution of intensity when the apparatus is made to turn in the same
direction and with the same velocity that the magnetization gives to
the jet, while, in the same circumstances, the rotation given to the
swivel by the jet produces a diminution of the current-intensity
from $5°$ to $3°$.   We can even, without employing the action of the
magnet, communicate a rotatory motion to the swivel directly, by
means of the rapid rotation of the bell-glass, in which it will meet
and cut the motionless jet several times without there resulting any
change in the intensity of the current.   If, however, the diminution
were owing to the cooling of the jet effected by its contact with the
swivel, we ought to observe it in this case—while if it proceeds
from the work done upon the swivel, as there is none in this expe-
riment, there ought to be no diminution : now this is just what does
take place*.

* The employment of the revolving table enables us to show in the most
direct manner that the division of the jet in its rotation under the influence
of the magnet is only an illusion, depending solely on the velocity of the
rotation and the continued impression resulting therefrom upon the retina.

The question, however, deserves a closer examination; and this we intend to give it. We will therefore not at present dwell on the consequences which may be deduced from our experiments—in particular as regards the constitution of the luminous gaseous thread which forms the jet, and which has a mechanical force so pronounced. We will merely observe that it has a marked analogy to that part of the electric discharge from the Ruhmkorff coil, in air under the ordinary pressure, which M. Perrot designated by the name *aureole*, and which he found susceptible of displacement by a simple mechanical impulse such as the breath.—*Comptes Rendus de l' Acad. des Sciences*, April 29, 1872, p. 1141.

ON THE ELECTRICAL CONDITION OF GAS-FLAMES.
BY JOHN TROWBRIDGE, ASSISTANT PROFESSOR OF PHYSICS.

Prof. H. Buff, of the University of Giessen, published in the *Annalen der Chemie und Pharmacie*, vol. lxxx. p. 1, and in the Phil. Mag. of Feb. 1852, an investigation of the electrical properties of flames. He reviews at first the different theories in regard to the subject; Becquerel, for instance, finds electric opposition in all directions in flames, which depends upon the difference of the temperature of the metals immersed in them. Pouillet recognizes a motion of electricity only from the interior to the exterior, and hence also from the base to the summit of the flame; Hankel, however, finds a motion the reverse of this in the flames produced by the ignition of spirits, and states that it is independent of the temperature of the immersed conductor.

Prof. Buff then gives the following as the results of his investigation:—

1. Gaseous bodies which have been rendered conductible by strong heating are capable of exciting other conductors, solid as well as gaseous, electrically.

2. When a thermo-electric circuit is formed of air, hydrogen, or carburetted hydrogen, alcohol vapour, charcoal, or, finally, a metal, whether combustible or incombustible, an electric current is developed, which proceeds through the air from the hottest place of contact to the less warm place.

3. The development of electricity which has been observed in processes of combustion, and particularly in flames, is due to thermo-electric excitation, and stands in no immediate connexion with the chemical process.

4. The products of combustion do not by any means, therefore, occupy the relation to the burning body which has been assumed by Pouillet; if positive electricity rises with the ascending gases, it is only in the degree in which the air exterior to the place of hottest contact is connected by a proper conductor.

In fact, if we mechanically cause the bell-glass, in which the jet is, to turn on its axis, the jet presents exactly the appearance of the spokes of a wheel when the rotation attains a certain degree of rapidity.

The following are the results which I have obtained in testing the electrical condition of the flame of a Bunsen burner with a Sir William Thomson's quadrant electrometer. The degrees given refer to the arbitrary divisions of the scale, upon which a spot of light is reflected from the mirror of the instrument.

Upon connecting the testing-plate of one pair of quadrants of the instrument with the flame, while the other pair were connected with the metallic burner and with the earth, the flame was found to be negatively electrified.

The following are some of the experiments, selected from a series that were made.

Exp. 1. Flame 12 centims. high; plate at the height of 7 centims. A negative indication of 130°, very steady.

Exp. 2. A platinum wire, substituted for the plate, and meeting the flame 3 centims. above the burner, gave a deflection of 30° in a negative direction.

Exp. 3. With the testing-plate just above the tip of the flame, the instrument showed a positive deflection of 70 to 80 degrees.

Exp. 4. With the testing-plate 5 millims. from the outer surface of the flame, on all sides, a feeble positive charge was obtained, the air in contact with the flame being apparently charged positively, the indication in no case exceeding fifty or sixty degrees on the scale of the electrometer.

Exp. 5. The metallic tip of the burner was found to be charged positively, giving an indication closely agreeing in the number of degrees with that corresponding to the negative indication of the flame. This indication was quite constant.

Exp. 6. When a glass tip was substituted for the metallic tip, no charge was found upon it. This was the case when any non-conducting body formed the tip.

Exp. 7. A glass tip having been substituted for the metallic one, a platinum wire was inserted below the orifice and carefully pushed upward until it occupied the centre of the interior cone of flame. A very feeble indication of negative electricity was the result.

While, with the Bunsen burner, the flame and the metallic tip are in decided electrical opposition, the one having a negative charge and the other a nearly equal positive charge, in spirit-flames the two opposite states recombine, the wick of the lamp and the fluid contained in the vessel connecting the two charges. The flame, therefore, merely takes the potential of the atmospheric electricity at the place where it is situated.

The electrical condition of the flame of a Bunsen burner when tested by a sensitive galvanometer gives in the main the same results as those obtained by Prof. Buff from spirit-flames. The quantity of electricity in the current passing from the flame to the tip is exceedingly small; whereas we have seen above that the terminal immersed in the gas-flame has a tension a little exceeding that of the negative pole of a Daniell's element.

The air in the room, at the time the above experiments were first performed, was charged positively to about the tension of the positive

pole of twelve Daniell's elements. The experiments were afterwards repeated, when the air in the room indicated a negative charge, with no difference in the results.

At the suggestion of Dr. Wilcott Gibbs, Rumford Professor, I tried the above experiments with a Bunsen blast lamp, by means of which I could increase the heat and the flow of air and gas at pleasure. Slight deviations in the scale-readings were obtained by this means : the flow of air appeared to affect the charge of electricity upon the metallic tip, rendering it less constant. The above experiments were in the main confirmed. The nature of the metallic plate submitted to the flame, and the degree to which it was heated, appeared to have a very slight influence upon the charge.

Sir William Thomson, in the Proceedings of the Literary and Philosophical Society of Manchester, March 1862, has a paper upon the electricity of the air in rooms. He finds that it is generally negative. By placing a spirit-lamp upon the prime conductor of an electrical machine, he was enabled to change the tension of the air from a positive to a negative state and the reverse. He carefully separates the results obtained from the *idioelectric* effect of the flame, which, he states, in no case gave a tension equal to either pole of a Daniell's element.

During the past winter observations made in the laboratory tend to confirm these views. I have, however, found on some days the air within strongly positive. The room is in the north-west corner of the building ; and there was a strong north-west wind blowing at the times this was observed. I noticed, also, while experimenting with the flame of a Bunsen burner placed near the water-dropper used by Sir William Thomson in investigating the electrical state of the atmosphere, that the positive charge of the air in the neighbourhood greatly decreased, and in some instances became feebly negative, by the presence of the flame.

The popular idea that great fires are followed by a change in the atmosphere inducing rain, does not seem to be unwarranted from an electrical point of view. The electricity of the air during cloudy and rainy weather is generally negative, or at the most feebly positive. The flames being negative would tend to change a strong positive charge in clear weather to a negative one, and thus bring the air to the state noticed in cloudy and rainy weather.

The following are the conclusions to which the above experiments lead :—

1. The flame of a Bunsen burner is negative, while positive electricity accumulates on the burner itself, if it is a good conductor. With orifices made of non-conductors, no charge was found upon the tip.

2. The stratum of air in contact with the outer cone of flame is slightly charged with positive electricity. The partly consumed gas of the interior cone is neutral.

3. The presence of flames tends to change the nature of the atmospheric electricity at the given place, reducing a positive tension to a feebly negative one.—Silliman's *American Journal* for July 1872.

ON THE PRIMARY SPECTRUM OF IODINE.   NOTE BY M. G. SALET.

The emission of a red light by the vapour of iodine strongly heated appeared to me so interesting that I was induced to study more closely the spectrum of that metalloid.

MM. Plücker and Hittorf did not succeed in producing with iodine, by means of Geissler's tubes, a spectrum of the first order corresponding to the absorption-spectrum ; I have been more fortunate by employing a *sheathed tube* ; for I have been able at will, and in the same apparatus, constructed entirely of glass, to obtain the line spectrum described by Plücker and also a new spectrum, of which the less-refrangible part represents, so to say, the negative proof of the fine absorption-spectrum so well studied by M. Thalén.   It is accompanied by excessively diffuse bands in the commencement of the blue and the end of the indigo.   These bands become more luminous when the tension of the vapour is increased ; but then the lines of the secondary spectrum appear.   The light of the tube is of a bronzy yellow in the cold ; it becomes violet-blue with heating.

To obtain the new spectrum, it is important to use an electric source of feeble tension, such as the induction-coil accompanied by a jar.   It is not very bright, unless particular skill is employed for its observation and the section of the narrow tube is presented to the spectroscope.   Each bright band, brought under the cross-wires of the telescope, is replaced by a black band when the vapour is illuminated from behind.

Here, then, is a fresh example of *multiple spectra*.   It cannot be supposed that the substance furnishing the new spectrum is a compound of iodine ; for it would be the same compound that gives the bands so well known of the absorption-spectrum ; in other terms, the characteristic coloration of iodine, that from which it derives its name, would be due to an impurity.

It therefore seems to me proved that one and the same elementary substance may have two spectra, as it may have two allotropic states. This was Plücker's old opinion.

It became interesting to know if the continuous spectrum of iodine heated to redness would show indications of the primary bands, as required by the theory of the proportionality of the emissive and absorptive powers.   By securing the most favourable conditions and employing strong dispersion, I have in fact succeeded in detecting the principal of them.—*Comptes Rendus de l'Acad. des Sciences*, July 8, 1872, pp. 76, 77.

ON A SIMPLE APPARATUS FOR THE PRODUCTION OF OZONE WITH ELECTRICITY OF HIGH TENSION.   BY PROF. ARTHUR W. WRIGHT.

Experiment has shown that in the production of ozone by electricity the maximum amount of oxygen is ozonized by the silent or glow discharge ; and most of the forms of apparatus by which this is affected are contrivances by which oxygen is made to flow slowly

through a space traversed by such a discharge. In v. Babo's apparatus, as well as in those of Siemens and Houzeau, the metallic conductors are separated by glass and a stratum of air. By inductive action of the charged metallic surfaces the intervening air becomes charged with electricity oppositely upon its two sides; and simultaneously with the discharge of the metallic terminals through the wire of the coil, a discharge takes place through the air, not in the form of sparks, but diffusely, producing a glow of purplish light, visible only in the dark.

These apparatus succeed best with electricity of comparatively low tension. In using the Holtz's electro-machine with them the discharge is apt to occur chiefly in the form of sparks through the air, or it may even traverse and perforate the glass, and the form of the apparatus must be varied to give the best results.

When the poles of the machine itself are separated to a sufficient distance, the electricity passes between them either in the form of a diffuse brush, spanning the whole interval, or with a very minute brush upon the negative pole, and a glow upon the positive, the intermediate space not being visibly luminous. This is the so-called dark or silent discharge, exhibiting the phenomena of the electric shadow when suitable objects are interposed, as described in a former paper[*]. When this occurs the strong odour shows that a considerable amount of the atmospheric oxygen is converted into ozone.

If this discharge is made to take place in an inclosed space through which air or oxygen can be driven, the ozonizing effect of the electricity is heightened and can be utilized. The apparatus which I have employed, and which has afforded very satisfactory results, consists of a straight glass tube about 20 centimetres long and having an internal diameter of 2·5 centimetres, the two ends being stopped with corks covered on the inner side with a thin coating of cement to protect them from the action of the ozone. Through the axis of each cork is inserted a glass tube of about 5 millimetres calibre, and 7 centimetres in length, having a branch tube inserted perpendicularly at the middle and long enough to permit a rubber tube to be slipped upon it. The outer ends of the tubes themselves are closely stopped with corks, through which are passed straight, thick copper wires carrying suitable terminals at their inner ends, and bent into a ring at the others. They are fitted so as to make tight joints, but to allow of motion in order to vary the distance between their inner ends. One of these wires carries a small ball; the other terminates in a disk with rounded edge, set perpendicularly to the axis of the tube, and so large as to leave an annular space of some two or three millimetres breadth around it. The gas is admitted through one of the branch tubes, and escapes from the other after having passed through the whole length of the tube.

In using the apparatus the wires must be connected with the poles of the machine in such a manner that the disk becomes the negative terminal, as this arrangement gives the greatest degree of expansion and diffuseness to the current. On turning the machine, and

adjusting the ball and disk to a proper distance, a nebulous aigrette surrounds the latter, quite filling the interval between it and the wall of the tube, while the part of the tube between the disk and ball is crowded with innumerable hazy streams converging upon the positive pole, or simply causing the latter to be covered with a faint glow. A current of air or oxygen sent into the tube must pass through this, and ozone is very rapidly produced and in great quantity. The condensers are of course not used with the machine when this apparatus is employed.

There appears to be an advantage in causing the oxygen to pass from the negative toward the positive within the tube; for the gas through which the discharge passes is transported in the contrary direction, as may be readily seen on bringing a candle-flame between the poles of the machine, or causing a thin column of smoke to rise through the polar interval. The flame and the smoke are deflected, and stream off towards the negative pole. If the gas should be admitted in the direction mentioned, there would be a tendency to obstruct its flow somewhat, and thus keep it longer under the influence of the electricity.

Some experiments which were made with the apparatus will give an idea of its efficiency. One hundred cubic centimetres of water were placed in an upright tube or test-glass, and into it were put 20 drops of strong indigo solution, causing it to assume a deep blue tint. Air was driven through the ozonizing tube, under a pressure of about three inches of water, and on issuing from it conveyed by a tube into the solution. When the electro-machine was put in operation, being turned with sufficient speed to give nearly its maximum effect, the solution completely lost its blue colour in less than four minutes. Blue litmus solution, under similar circumstances, became pale pink, but required a considerably longer time for the change.

When Schönbein's test solution is employed, the deep blue colour is immediately produced; but the solution is too thick to work well if the starch has been heated considerably, or for a long time, in making it. A better proportion to take is one part of potassic iodide by weight, ten parts of starch, and five thousand parts of water. This forms a milky solution sufficiently mobile to mix well when the ozonized air bubbles through it. When 100 cubic centims. of this solution were used, and air passed through the apparatus as before, the blue colour appeared at once on application of electricity, and in thirty seconds it was deeply coloured.

With dry oxygen the effects were much more rapid and remarkable. 100 cubic centims. of the solution were used as before. The instant the machine was put in action the liquid about the end of the delivery-tube became deep blue; and in from ten to fifteen seconds the whole had acquired a uniform and intense blue colour.

The summer moisture having interfered somewhat with the effective working of the electro-machine, there has been no opportunity to determine the percentage of ozone produced in this manner, but it appears to be very large. When dry oxygen is passed through

the tube very slowly, the issuing gas, when inhaled, produces a painful burning sensation in the lungs and causes violent coughing, which persists for a considerable time.

When oxygen is used, it is found that the electrodes must be separated to a much greater distance than is necessary for air, otherwise sparks pass and destroy a large proportion of the ozone already formed. With air, the direct spark in the apparatus could not be made to pass over an interval of more than 7 centims.; but in oxygen they did not cease until the poles were separated about 11·5 centims. When the tube was filled with air and the poles were 7 or 8 centims. apart, the discharge was of the silent kind; but on admitting oxygen it immediately took the form of direct sparks.

The quantity of the solution used in these experiments was much greater than would be needed in order to exhibit the characteristic reactions of ozone to an audience of moderate size. One half or one third of the amount would be quite sufficient; and the time required for the reaction would be proportionally shorter. The great quantity of the ozone, as well as the ease and rapidity with which it is produced, render the apparatus especially serviceable for use in the lecture-room.—Silliman's *American Journal* for July 1872.

ON A SINGULAR APPEARANCE OF MAGNESIUM IN THE CHROMO-SPHERE OF THE SUN. BY M. TACCHINI, IN A LETTER ADDRESSED TO M. FAYE.

I have just observed a phenomenon which is altogether new in the series of my observations. I had already, from the 6th of May, discovered in the sun regions remarkable by the presence of magnesium—very extensive regions, *i.e.* comprising arcs of 12–168°, whilst preceding observations give arcs of only 66° at the most.

At the meeting of our Société des Sciences on the 18th of May, I presented a drawing of the entire margin, executed May 6, with indications of the position of the magnesium, and added some special considerations on the observations of the following days. Although the extent of the magnesium showed itself always considerable, I noticed that the longest and most characteristic streaks were found on the western limb, as I had proved also, for the preceding observations, in the *Bulletin de l'Observatoire*, 1871, No. 9. This predilection, as it were, for the western limb was difficult to account for; consequently it was of interest to continue the study of the magnesium-lines all round the margin; and this I am doing every morning when the atmosphere is clear and calm.

At last, on the 18th of June, I was able to verify the presence of magnesium in every part of the solar margin—that is to say, that the whole chromosphere was invaded by vapours of that metal. To this general ebullition (which accords so well with your theory) corresponded an absence of protuberances, which seems to me very natural; on the contrary, the flames of the chromosphere were very pronounced and brilliant. I seemed to be witnessing the renewal of the surface of our grand source of light.

The more pronounced and brilliant the flames, the brighter and

broader appeared the magnesium-lines. At 288° very brilliant and characteristic flames were observed; I said at the time, to some persons who happened to be present, that at that point there must certainly be a fine facula. In fact, on examining the sun by projection, we discovered at the place indicated a very luminous facula which was precisely on the limb of the sun; this was one of those verifications which I have repeated by myself so many times, with perfect accordance. The granulations showed very distinct; and on the contour of the disk the number of small faculæ was everywhere in accordance with the presence of magnesium.

At each position of the spectroscope I likewise noted the relative intensity of the lines; and I many times observed that the variations of breadth of the lines corresponded perfectly with the variations of luminous intensity of the chromospheric flames observed through the line C.

The great abundance of magnesium still continues to be manifested, but no longer on the whole margin.

These observations seem to me to demonstrate that we ought to admit, not local eruptions, but rather complete eruptions—that is to say, a mixture of certain metallic vapours with the chromosphere, a mixture which extends to every part of the surface of the sun, which must consequently be in the gaseous state.

More than one person have told me that at the present time the light of the sun has not its ordinary appearance; and we at the observatory have thought we noticed the same thing. This change should be attributed to the magnesium.

Height of the Chromosphere at each position of the Spectroscope, June 18, 1871.

N. 0° 15	W. 90° 14	S. 180° 12	E. 270° 12
6 13	96 13	186 10	276 12
12 13	102 14	192 10	282 13
18 13	108 15	198 10	288 12
24 17	114 13	204 13	294 12
30 22	120 14	210 11	300 12
36 11	126 13	216 14	306 12
42 16	132 13	222 13	312 12
48 13	138 13	228 13	318 13
54 13	144 13	234 13	324 12
60 11	150 15	240 13	330 13
66 12	156 14	246 13	336 13
72 13	162 22	252 13	342 13
78 14	168 12	258 13	348 13
84 14	174 12	264 13	354 15
W. 90 14	S. 180 12	E. 270 12	N. 0 15

There is, then, at the south pole a marked depression; it is the contrary at the north pole. Hence it is natural to ask, Does the greatest activity at the surface of the sun correspond to the greatest number of protuberances, or to the greatest extension of the magnesium-regions with the exaggerated flames of the chromosphere? This is a question which I cannot yet answer.—*Comptes Rendus de l'Acad. des Sciences,* July 1, 1872.

XXI. *On Differential Galvanometers.*
By Louis Schwendler, *Esq.*[*]

THERE is one very interesting question connected with the construction of these instruments which, as far as I know, has not yet been answered, and which is of sufficient practical importance to form the subject of an investigation.

This question may best be put as follows:—

A certain battery of given electromotive force and given internal resistance has to supply the two coils of any differential galvanometer with a current; what must be the resistance of either coil in order to obtain the most delicate reading when measuring a given resistance?[†]

The solution of this problem in its most general form would naturally be extremely intricate, and could not be effected without tedious calculation; but there is one special case where it is comparatively easy to determine the law which connects the resistance of the coils with the external resistances to be compared, in order to have the greatest sensitiveness of the instrument.

Suppose, for instance, that the two coils of a differential galvanometer have equal resistances and equal magnetic momenta, and further, that the battery which supplies the two coils with current has an internal resistance sufficiently small to allow of its being neglected against the resistances to be compared. Then, on account of the battery resistance being so small, it

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[*] From the Journal of the Asiatic Society of Bengal, vol. xli. part 2, 1872. Communicated by the Author.

[†] In the Philosophical Magazine of May 1866 and January 1867 I solved a similar question, viz. the proper resistance of the galvanometer to be employed when testing by Wheatstone's balance; and the result of that investigation has led me to examine the present question.

follows that the current through one coil is entirely independent
of the total resistance in the other; and as the two coils are sup-
posed to have equal magnetic momenta and equal resistances,
balance can only be established by the currents becoming equal;
that is to say, at or near balance each coil must receive a current

$$C = \frac{E}{g + w},$$

where g is the unknown resistance of either coil, w the resistance
to be measured, and which is supposed to be known, and E the
given electromotive force of the testing battery.

At balance the diagram of this differential galvanometer is
therefore represented by fig. 1.

Fig. 1.
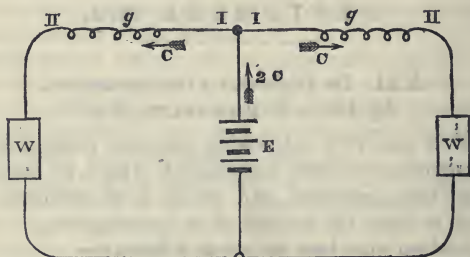
Now, as far as the magnetic effect of the two coils is con-
cerned, we may substitute for the parallel circuit (fig. 1) the
simple circuit (fig. 2) if we only reverse the magnetic action of
one of the two coils (say the right one).

Fig. 2.
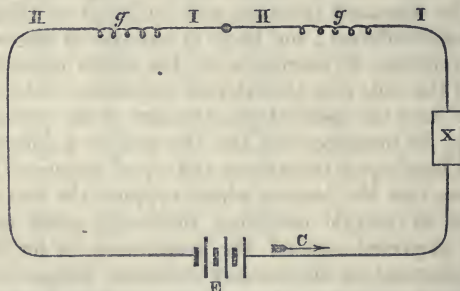
And in order to have in this case for the same electromotive
force E the same current C flowing through the coils as before
(see fig. 1), we must necessarily introduce a resistance x; hence

$$C = \frac{E}{g + w} = \frac{E}{2g + x};$$

therefore $$w = g + x. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(I.)}$$

But to obtain the maximum magnetic effect in any single circuit (fig. 2), it is necessary that the resistance of the coil should be equal to the total external resistance*; and therefore in this case (fig. 2)

$$x = 2g. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(II.)}$$

Eliminating x from equations (I.) and (II.), we have

$$g = \frac{w}{3}. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad \text{(I.)}$$

To obtain the most delicate reading with a differential galvanometer, the two coils of which have equal magnetic momenta and also equal resistances, the resistance of each coil should always be the third part of the resistance to be measured.

This relation is so exceedingly simple that at first I thought it must be a well-known one, and that I only was unacquainted with it. However I have since carefully read the literature on the subject, and I find the above law nowhere stated; and as a further proof of its being new, I may add that none of the differential galvanometers with which I have had occasion to deal fulfil it. That this relation is of the greatest importance in the construction of differential galvanometers cannot be doubted; and I have accordingly thought it worth while to bring my investigation before the Society.

Solution of the Problem in its most general form.

Fig. 3 gives the diagram of a differential galvanometer in its general form. w and w' are the two resistances to be compared, and which we suppose to be given; E is the given electromotive force of the testing battery, and f the total resistance in the battery branch; g and g' are the resistances of the two coils, and their values are to be determined under the condition that the reading, when near balance, is most delicate, *i. e.* that the slightest variation in w or w' causes the greatest possible variation in the deflection of the needle.

Fig. 3.
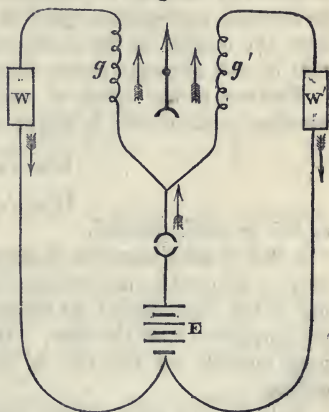
* This law holds good, as can easily be shown, for any number of coils connected into a single circuit, no matter whether the magnetic effects of these coils have the same or opposite sign with respect to a given magnetic point.

The magnetic moment of the coil g, when a current G passes through it, may be designated by Y; and the magnetic moment of the coil g', when a current G' passes through it, may be called Y'. Both these magnetic momenta are taken with respect to the same needle, or system of needles; and we may suppose that neither Y nor Y' alters perceptibly when the needle, or system of needles, slightly alters its position towards the coils, which are supposed to be fixed. (This condition will be fulfilled as closely as possible near balance, when the needle is approximately always in the same position with respect to the coils; and it is only for such a case that the following investigation is of any practical interest.)

According to the principle of the differential galvanometer, we have

$$a^\circ \varpropto Y - Y',$$

where a represents the deflection of the needle before balance is arrived at, and which may be positive, zero, or negative, depending on the relative strength of the currents which at the time are acting through the coils, on the relative position of the needle towards the coils, and on the shape and size of the latter.

Approximately we have further,

$$Y = m \, U \, G,$$
$$Y' = m' \, U' G',$$

U and U' being the number of convolutions in the coils g and g' respectively, and m, m' representing the magnetic momenta of an average convolution (one of mean size and mean distance from the needle) in the coils g and g' respectively, when a current of unit strength passes through them.

Further, as the space of each coil to be filled with wire of constant conductivity is given, we have

$$U = n \sqrt{g},$$
$$U' = n' \sqrt{g'},$$

as can be easily proved.

n and n' are quantities independent of g and g' so long as it may be allowed to neglect the thickness of the insulating covering of the wire against its diameter, which for brevity's sake we will suppose to be the case. With this reservation n and n' depend entirely on the size of the coils and on the manner of coiling.

Substituting these values, we get

$$a^\circ \varpropto mn \sqrt{g} G - m'n' \sqrt{g'} G', \quad . \quad . \quad . \quad . \quad \text{(I.)}$$

which general expression for the deflection we may write in two

different forms—either

$$a^\circ \propto mn \sqrt{g} \left(G - \frac{m'n'}{mn} \frac{\sqrt{g'}}{\sqrt{g}} G'\right), \quad \cdots \quad (I.)$$

or

$$a^\circ \propto m'n' \sqrt{g'} \left(\frac{mn}{m'n'} \frac{\sqrt{g}}{\sqrt{g'}} G - G'\right), \quad \cdots \quad (I'.)$$

which means that any deflections observed may of course be considered due to either coil. In the first form (equation I.) it is considered due to the coil g when a current $G - \frac{m'n'}{mn} \frac{\sqrt{g'}}{\sqrt{g}} G'$ flows through it; in the latter form (equation I'.) it is considered due to the coil g' when a current $\frac{mn}{m'n'} \frac{\sqrt{g}}{\sqrt{g'}} G - G'$ flows through it.

Now, considering that the same battery E has to supply the current to both the coils, we have

$$G = E \frac{g' + w'}{N}$$

and

$$G' = E \frac{g + w}{N},$$

where $N = (g+w)(g'+w') + f(g+w+g'+w')$.

Thus, substituting in (I.) and (I'.), we get either

$$a^\circ \propto mnE \frac{\sqrt{g}}{N} \overbrace{\left(g' + w' - \frac{m'n'}{mn} \frac{\sqrt{g'}}{\sqrt{g}} (g+w)\right)}^{\Delta}, \quad \cdots \quad (I.)$$

or

$$a^\circ \propto m'n'E \frac{\sqrt{g'}}{N} \overbrace{\left((g'+w') \frac{mn}{m'n'} \frac{\sqrt{g}}{\sqrt{g'}} - (g+w)\right)}^{\Delta'}; \quad \cdots \quad (I'.)$$

and either Δ or Δ' is the factor which at balance becomes zero.

The coefficient $\frac{m'n'}{mn} \frac{\sqrt{g'}}{\sqrt{g}}$ means, therefore, nothing else than what is generally called the constant of the differential galvanometer, *i. e.* the number by which the total resistance in one branch of the differential galvanometer has to be multiplied in order to obtain the total resistance in the other branch, when balance is established. This constant of the differential galvanometer is a given function of g and g', the resistance of the coils; and as g and g' are to be determined, by being variable, it cannot be considered a constant in this investigation. But the

factor $\frac{m'n'}{mn}$ is entirely independent of any of the resistances, it represents what may appropriately be called the "*mechanical arrangement*" of the differential galvanometer, and may be designated by p. It must be borne in mind that p represents an absolute number, which theoretically may be any thing with the exception of 0 and ∞. If p had a value equal to either of these two limits, the instrument would be a simple galvanometer with a shunt, and not a differential galvanometer.

The deflection a may now be written more simply as follows:—

$$a^\circ \propto K \frac{\sqrt{g}}{N} \overbrace{\left(g' + w' - p \frac{\sqrt{g'}}{\sqrt{g}} (g + w) \right)}^{\Delta} = K \frac{\sqrt{g}}{N} \Delta, \quad . \quad (I.)$$

or

$$a^\circ \propto K' \frac{\sqrt{g'}}{N} \overbrace{\left(\frac{g' + w'}{p} \frac{\sqrt{g}}{\sqrt{g'}} - (g + w) \right)}^{\Delta'} = K' \frac{\sqrt{g'}}{N} \Delta', \; (I'.)$$

K and K′ being independent of g and g', and also of w and w'.

N is a known function of all the resistances in the differential circuit.

Δ and Δ' are similar functions of g and g', w and w', and which both become zero at balance.

For the further investigation only one of the two possible expressions of a will be used, viz. equation (I.).

$$a^\circ \propto K \frac{\sqrt{g}}{N} \Delta. \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (I.)$$

Differentiating this expression with respect to w', the external resistance belonging to the coil g', we get

$$\frac{da}{dw'} = K \left\{ \frac{\sqrt{g}}{N} - \frac{\Delta R \sqrt{g}}{N^2} \right\},$$

where

$$R = \frac{dN}{dw'};$$

or the variation of the deflection a, when w' varies, is

$$\delta a = K \left\{ \frac{\sqrt{g}}{N} - \frac{\Delta R \sqrt{g}}{N^2} \right\} dw' = K \phi \, dw'.$$

Now it is clear that the instrument is most sensitively constructed when, for the slightest variation in w', the variation in a is greatest. This will be the case if the factor $\phi = \frac{\sqrt{g}}{N} - \frac{\Delta R \sqrt{g}}{N^2}$ is as great as possible. This factor ϕ is a known function of the

resistances in the circuit; and as w and w' are given, ϕ can only be made a maximum with respect to g and g', the resistances of the two coils.

Thus our physical problem is reduced to the following mathematical one:—

A function ϕ containing two variables is to be made a maximum, while the two variables are connected with each other by the relation

$$\Delta = g' + w' - \frac{p\sqrt{g'}}{\sqrt{g}}(g+w),$$

Δ being a constant with respect to g and g' and becoming zero at balance.

Solving this question (relative maxima), we get

$$\frac{(w-g)\ (w'+g') + f(w+w'+g'-g)}{p\ (g-w)\ g'} = \frac{2\ (g+w+f)}{2\sqrt{g}\sqrt{g'} - p\ (g+w)}, \text{(II.)}*$$

* To some of the readers a more detailed working out of the mathematical problem may perhaps be welcome; and as this will also prove to be an easy control over the equations (II.) and (II'.), I will give it here in a somewhat condensed form. We had

$$a^\circ \propto K \frac{\sqrt{g}}{N}\Delta, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \text{(I.)}$$

where K represents a constant, *i. e.* a quantity independent of any of the resistances in the differential circuit (fig. 3), while $\Delta = g' + w' - p\dfrac{\sqrt{g'}}{\sqrt{g}}(g+w)$, *i. e.* a resistance which at balance becomes $=0$; and further,

$$N = (g+w)(g'+w') + f(g+w+g'+w').$$

Differentiating a with respect to w', and remembering that $\dfrac{d\Delta}{dw'} = 1$, and substituting $\dfrac{dN}{dw'} = R$, we have

$$\frac{da}{dw'} = K\left\{\frac{\sqrt{g}}{N} - \Delta\frac{R\sqrt{g}}{N^2}\right\};$$

$$\therefore\ \delta a = K\underbrace{\left\{\frac{\sqrt{g}}{N} - \frac{R\sqrt{g}}{N^2}\right\}}_{\phi}\delta w';$$

$$\therefore\ \delta a = K\phi\delta w'.$$

Thus the variation of a is always directly proportional to ϕ, a known function of g and g'; and to make δa for any $\delta w'$ as large as possible, we have to make ϕ a maximum with respect to g and g', while g and g' are connected by the following equation:

$$\Delta = g' + w' - p\frac{\sqrt{g'}}{\sqrt{g}}(g+w), \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \text{(I.)}$$

p being a constant with respect to g and g', as also is Δ.

We have therefore to deal here with a relative maximum; and in accordance with well-known rules we have to form the following partial differ-

which equation, with the other,

$$g' + w' - p \frac{\sqrt{g'}}{\sqrt{g}}(g+w) = \Delta = 0, \quad . \quad . \quad . \quad (1)$$

gives all that is required to determine g and g'; and the values thus obtained would be those which would make the reading

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ential coefficients :—

$$\frac{d\phi}{dg} = \left\{ \frac{N - 2g\dfrac{dN}{dg}}{2\sqrt{g}N^2} - \frac{R\sqrt{g}\dfrac{d\Delta}{dg}}{N^2} + \Delta S \right\},$$

$$R = \frac{dN}{dw'} = g + w + f,$$

$$S = \frac{\sqrt{g}}{N^2} \left\{ \frac{2R\dfrac{dN}{dg}}{N} - \frac{dR}{dg} - \frac{R}{2g} \right\},$$

$$\frac{d\phi}{dg'} = -\left\{ \frac{\sqrt{g}\dfrac{dN}{dg'}}{N^2} + \frac{R\sqrt{g}\dfrac{d\Delta}{dg'}}{N^2} + \Delta S' \right\},$$

$$S' = \frac{\sqrt{g}}{N^2}\left(\frac{dR}{dg'} - \frac{2R\dfrac{dN}{dg'}}{N} \right),$$

$$\frac{d\Delta}{dg} = \frac{w-g}{g}\frac{p}{2}\frac{\sqrt{g'}}{\sqrt{g}},$$

$$\frac{d\Delta}{dg'} = \frac{2\sqrt{g}\sqrt{g'} - p(g+w)}{2\sqrt{g}\sqrt{g'}}.$$

At or near balance, when Δ is $=0$ or very small, the terms ΔS and $\Delta S'$ in the respective differential coefficients are to be neglected, because neither S nor S' becomes infinite for any finite values of g and g'.

Thus we have approximately,

$$\frac{d\phi}{dg} = \frac{N - 2g\dfrac{dN}{dg}}{2\sqrt{g}N^2} - \frac{R\sqrt{g}\dfrac{d\Delta}{dg}}{N^2} = P - Q.$$

$$\frac{d\phi}{dg'} = -\left\{ \frac{\sqrt{g}\dfrac{dN}{dg'}}{N^2} + \frac{R\sqrt{g}\dfrac{d\Delta}{dg'}}{N^2} \right\} = -(P' + Q').$$

Further, we will substitute

$$\frac{d\Delta}{dg} = \alpha,$$

$$\frac{d\Delta}{dg'} = \beta;$$

thus we have the following differential equation :—

$$(P - Q)dg - (P' + Q')dg' + \lambda(\alpha dg + \beta dg') = 0,$$

most delicate near balance, when the variation takes place in w', *i. e.* the external resistance belonging to the coil g'.

If, instead of differentiating the expression for a with respect to w' by using the expression (I.), we had done so with respect to w by using the expression (I'.), we should have obtained in a similar way the following relation between g and g',

$$\frac{(w'-g')(w+g)+f(w+w'+g-g')}{\frac{g}{p}(g'-w')} = \frac{2(g'+w'+f)}{2\sqrt{g}\sqrt{g'}-\frac{g'+w'}{p}}, \quad \text{(II'.)}$$

which equation, connected with the other,

$$\frac{g'+w'}{p}\frac{\sqrt{g}}{\sqrt{g'}}-)g+w=\Delta'=0, \quad . \quad . \quad . \quad (1)$$

gives all that is necessary to determine g and g', being those values which would make the reading at or near balance most

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λ being the undetermined factor. From this equation we have

$$P-Q+\lambda\alpha=0$$

and

$$-(P'+Q')+\lambda\beta=0;$$

or, λ eliminated,

$$-\frac{P-Q}{\alpha}=\frac{P'+Q'}{\beta};$$

but we have always

$$\frac{Q}{\alpha}=\frac{Q'}{\beta};$$

thus we have as final equation,

$$-\frac{P}{\alpha}=\frac{P'}{\beta};$$

or, the value for P, P', α, and β substituted, we have

$$\frac{N-2g\frac{dN}{dg}}{pg'(g-w)}=\frac{2\frac{dN}{dg'}}{2\sqrt{g}\sqrt{g'}-p(g+w)};$$

further substituting

$$\frac{dN}{dg}=g'+w'+f,$$

$$\frac{dN}{dg'}=g+w+f,$$

and reducing as much as possible, we have

$$\frac{(w-g)(w'+g')+f(w+w'+g'-g)}{p(g-w)g'}=\frac{2(g+w+f)}{2\sqrt{g}\sqrt{g'}-p(g+w)}, \quad . \quad \text{(II.)}$$

which is the equation (II'.) as given above.

In quite a similar manner equation (II'.) can be found; it must only be remembered that it is more simple to use expression (I'.) for the purpose than (I.).

sensitive when a variation in w, the external resistance belonging to coil g, takes place.

Now it is clear that equations (II.) and (II'.) are not necessarily identical, as long as p does not fulfil certain conditions, and therefore the first set of equations (II.) and (1) may give entirely different values for g and g' from those obtained from the second set (II'.) and (I.), which means that a simultaneous maximum sensitiveness with respect to an alteration of the external resistance w or w' in either of the two differential branches, is *not* always possible. The following very important and interesting question, therefore, remains to be solved.

What general condition must be fulfilled in the construction of any differential galvanometer in order to make a simultaneous maximum sensitiveness possible, with respect to an alteration of external resistance in either of the differential branches?

<div style="text-align:center">[To be continued.]</div>

XXII. *On an Electrical Experiment with an Insulated Room.* *By* F. C. Webb, *M. Inst. C.E.**

SOME ten years ago, in a series of articles in 'The Electrician,' afterwards published in book-form, I submitted some new views on the explanation of the action which takes place in various well-known electrical phenomena. I endeavoured to prove by reasoning that the ordinary explanations given of the discharge of a charged conductor by communication with the ground were erroneous; and I suggested an experiment with an insulated room as a means of proving the fallacy of these ordinary explanations of discharge &c. This experiment I performed in May 1869 at Keyham Dockyard; and I think the results are worth placing on record.

To make the bearing of the experiment intelligible, however, it is necessary to recapitulate a little of what I have before urged.

It is generally stated in books on electricity, that, when an electrical machine has its rubber to earth, on the action of the machine the prime conductor becomes charged positively and the equal quantity of negative electricity which is generated is lost in the earth—and that when the prime conductor is then placed in contact with earth, its discharge is merely the consequence of its sharing its electricity with the earth, the proportion remaining on the conductor being, in virtue of the almost infinite size of the earth as compared with the conductor, almost infinitely small and consequently quite inappreciable.

I will not weary your readers by quoting, but will merely refer them to:—Professor Daniell's 'Chemical Philosophy,' p. 241;

Lardner and Walker, p. 248; De la Rive, vol. i. p. 3; Ganot, translated by Atkinson, p. 536.

When, therefore, the ordinary experiment of charging the prime conductor is performed in an insulated room, according to the theories which I deny, the negative electricity being unable to share itself with the earth, some differences in the effects produced should be attained. The negative electricity, for instance, if it tends to flow to the earth, should influence an electrometer placed in contact and exterior to the room. Again, if the room is connected to the earth during the charging of the prime conductor and is afterwards insulated, the prime conductor, if touched to the room, ought not to be discharged, since it can only share its electricity with the room instead of with the almost indefinitely large surface of the earth.

Now I argue that when electricity is produced, at any rate by an artificial means, the negative and positive are not only always produced in equal quantities, but remain each as much in abeyance as the other, and that complete discharge always consists in the recombination of equal quantities of opposite electricity. Thus when a conductor is charged, say, positively from a plate-machine having the rubber connected to the earth, the negative electricity has no tendency to flow to and distribute itself equally over the earth, but distributes itself principally on the nearest conducting-surfaces to the positively charged surface; and when the surface to which the negative pole is attached entirely bounds the dielectric which separates it from the positively charged surface, the negative electricity is entirely distributed on that surface, none flowing to any other part of the earth. When these are joined by a conductor, discharge occurs through the recombination of the exactly equal quantities of electricity previously produced.

The experiment to which I allude was performed in H. M. Dockyard, Keyham, where I was engaged in repairing a part of the Persian-gulf cable which had been thrown overboard from the ship 'Calcutta,' and recovered and landed under my superintendence. The insulated room was erected in the large glass-covered quadrangle of the dockyard. I intended to make a series of experiments, carefully recording every step. After a few preliminary experiments, however, of which unfortunately no record was kept, our regular work had to be suddenly pushed on; and before any series of experiments could be made and recorded, the whole apparatus had to be removed. Thus I am only able to write from memory; but Mr. Herbert Taylor (one of Mr. Latimer Clark's assistants, and a well-known electrician) was associated with me at the time and performed the experiments with me. I have submitted this article to him, and he indorses it as a correct statement of what occurred.

The room was about 8 feet by 9 and about 8 feet high. The floor was of wood, and the sides of wooden framework covered with calico and with pieces of tinfoil pasted about it to make it a good conductor. It had a door and two windows of wire gauze. A small table was placed in it, and a frictional electrical machine was placed on this. The room was suspended by four double parts of half-inch-round gutta-percha band to a wooden frame, the floor being about 4 feet from the ground. The gutta percha was covered with paraffin; and the whole room was tested for insulation with five hundred Daniell's elements with a delicate astatic Thomson's reflecting-galvanometer, and gave no perceptible loss. A Peltier electrometer was placed on the ground outside; and a wire from the brass knob of this was connected to the gauze of the window. The table was connected with the tinfoil that was pasted about the surfaces of the room, so that when the rubber or prime conductor was connected to the table it was in connexion with the sides of the room. Thus arranged, the machine acted to all appearance exactly the same as in an uninsulated room. When the rubber was connected to the table, on turning the glass disk the prime conductor was charged so as to give off sparks the same as when the room was uninsulated; and the conductor was completely discharged when touched to the insulated room. Not the slightest effect was produced on the electrometer, even when sparks were flashing from the prime conductor to the wire gauze of the windows to which the wire from the electrometer was attached. Connecting the room to the earth made no difference. A sphere about a foot in diameter was then charged when the room was insulated, and then handed out by an insulating handle, when the electrometer immediately diverged to about $50°$, showing that the outer surface of the room had become negative. On taking the sphere back into the room, the electrometer fell to zero.

These results agree exactly with what I have suggested would occur under such circumstances in my treatise on Electrical Accumulation published in 1862; and it is impossible to reconcile these results with the explanation of the action that takes place in charging and discharging as given and republished in most works. Take, for instance, the following from Noad's Student's Text-book:—" Thus, in order to get any development of electricity, there must be either with the rubber or with the prime conductor, electrical communication with the earth as the great natural reservoir of electricity." (P. 29.)

This is about the same language as that used by Lardner, De la Rive, Daniell, Gavarret, and later by Deschenelle.

According to the views I have advocated, the accumulation on the prime conductor depends on the resistance of what I have termed the inductive circuit, which in the case of charging the

prime conductor with the rubber connected to surrounding objects, consists simply of the resistance of the dielectric separating the prime conductor from the surrounding objects, and is therefore the same whether the room is connected to earth or not. More complete views on this subject will be found in what I have written before, both in my ' Electrical Accumulation' and in a paper " on Inductive Circuits, or the Application of Ohm's Law to Problems of Electrostatics," in the Philosophical Magazine for May 1868.

Dr. Ferguson, in his 'Electricity,' seems to have a tendency to discard the older explanations; but at times his explanations appear to me incomplete, so that it is impossible to form an opinion as to what his theory is as regards the actions that take place.

The following paragraph, however, appears to me remarkable, as it seems to show a tendency towards the theory which I have advocated :—

"We have hitherto taken no notice of the —E that, for instance, is said to be lost in the ground when glass is charged positively. Now it may be lost, and the —E induced by the glass on surrounding conductors may be new —E induced by it. But it is also possible, *nay, even probable*, that this —E is none other than the —E said to be lost.* If this be the case, the ground acts as much on the glass as the glass on the ground; and the action *is precisely the same as in a galvanic circuit,* when the polarization proceeds in opposite ways in two opposite directions, the action of the one strengthening the action of the other."

Here we have stated as probable what I have urged ten years ago, and which I think I have now demonstrated by experiment.

Dr. Ferguson adds, "However, it makes no practical difference; and it is simpler to suppose the insulated body to be the one centre of force." With this last paragraph I cannot agree, as it will be found that, by considering problems of electrostatics by means of the theory of inductive circuits, many can be solved which cannot be explained by an allusion to the earth as a common reservoir. The experiment with the insulated room is only one of them. I maintain that no electricity is ever lost in the ground or drawn from the ground as a reservoir. All electricity generated, if we consider it as a quantity, is produced in equal quantities, and can only be neutralized by an equal quantity of electricity of the opposite name.

Thus when the sphere in the insulated-room experiment was charged positively from the prime conductor, an equal quantity of negative went to the room. When the sphere was taken out of the room, this negative went to the outside of the room at

* The italics are mine.—F. C. W.

the same time that the external conductors to the room became polarized, so that there remained on external objects a charge of negative opposing the positive on the sphere, and a charge of positive opposing the negative on the external surface of the room.   By connecting the sphere to the earth, the sphere's charge and its opposing charge on surrounding objects neutralize each other, leaving the room negative to a positive charge in the earth, which, when touched together, neutralize each other.   Or if the sphere is touched to the room, the positive of the sphere neutralizes the negative on the room, the negative and positive charges in surrounding objects at the same time recombining also.   Or when the sphere was taken back into the room, the negative returned to the inside of the room, the positive and negative which were induced on external conductors by the sphere and room when the sphere was external to the room recombining.

It is to be regretted that circumstances prevented me, as I have stated, from performing a regular series of experiments with the insulated room ; nevertheless, as the experiment requires space and apparatus not always attainable, I have thought that the experiments, even so far as they went, are worthy of record, even if they do not carry the conviction which my arguments have for so long failed to establish amongst the authors of our text-books on electricity.

XXIII. *On the Nature of Electricity*.   *By* M. E. Edlund.
[Concluded from p. 100.]
Part II.

IN the first part of this memoir we have endeavoured to show that both electrostatic and electrodynamic phenomena can be explained with the aid of the luminiferous æther.   The same basis of demonstration will serve to explain to us some of the other principal properties of the galvanic current.

4. *Phenomena of galvanic induction.*—A molecule m' is at rest if it is equally repelled on all sides by the surrounding æther. Suppose now that, from any cause whatever, the æther has been compressed towards a point a in the vicinity of m' ; the repulsion exerted on this side upon m' will necessarily be greater than on the others.   In consequence, the molecule m', not being able to keep intact its state of equilibrium, will inevitably seek to move away from the point a.   It will be the same with all the molecules within the sphere of action of the compressed æther.   The consequence will be a rarefaction of the æther in the vicinity of a. The æther at a greater distance from a, the density of which has therefore undergone no sensible modification, seeks now to bring

back towards that point the æther which is in the vicinity of *a*. As soon as the rarefaction about *a* has reached a certain limit, the molecules enter a new state of equilibrium, which they preserve as long as the increase of density at *a* continues. If now the increase suddenly ceases, the molecules about *a* resume their primitive equilibrium, and in this case travel, although in the opposite direction, the same path as when the density was increasing.

A corresponding modification must be effected in the state of equilibrium of the surrounding molecules if the æther at *a* be rarefied instead of compressed; but in this case the direction of the motion of the molecules is the reverse of what it was in the preceding: they approach *a* at the commencement of the rarefaction, and recede therefrom when it ceases; and the amount of displacement is the same in the approach as in the recession. It is moreover evident that the modification of the state of equilibrium of a molecule, or the amount of its displacement, does not depend exclusively on the modification of the repulsion of the æther which surrounds it to a certain distance, but depends also on the facility with which it moves—or, in other terms, on the resistance to conduction as well as on the action of the nearest molecules. We have admitted, in the first part of this memoir, that the action of one molecule upon another varies inversely as the square of the distance. As we also indicated, this rule only applies where the molecules are at a sufficient distance from each other. If the molecules be in contact, or at a molecular distance from one another, the law of repulsion will perhaps be different —a circumstance which in no way affects the present consideration.

It is obvious that the æther molecules about *a* will change their position of equilibrium if, from any cause whatever, the repulsion exerted upon them by the æther of *a* be modified without becoming denser or rarer. Now the æther of *a* being set in motion will produce a modification of this kind. If, then, that æther be set in motion, the molecules of the surrounding æther will be displaced, and will remain in their new positions as long as the æther of *a* continues its motion without change. The instant the motion ceases, the molecules return to their original positions of equilibrium.

Such, in our opinion, is the cause of galvanic induction. When a galvanic current commences in the vicinity of a closed circuit, the positions of equilibrium of the æther molecules are changed, not only in the closed circuit, but also in the insulating medium encompassing it; and the induction-current is simply the passage of the molecules from the first position of equilibrium to the second. The new state of equilibrium of the æther in the closed

circuit is not determined exclusively by the direct action exerted by the inducing current upon it, but also by the modification of the state of equilibrium in the æther of the surrounding insulating medium. As soon as the inducing current ceases, the æther molecules return to their primitive position of equilibrium, and consequently we have in the closed circuit an induced current equal in intensity, but opposite in direction, to the former. When an inducing current is brought near or removed from a closed circuit, the effect is evidently the same as when a current commences or ceases in a circuit at rest. Although no induced current properly so called is observed in the insulating medium, since the great resistance to conduction impedes the origination of such a current, we have nevertheless no reason to suppose that the æther molecules therein remain in a state of perfect repose: the positions of equilibrium are modified there also, since experiment has demonstrated that no substance can be considered absolutely nonconducting.

If two molecules of æther, m and m', are at rest at the distance r one from the other—according to what has been said before, their mutual repulsion is $-\dfrac{mm'}{r^2}$. For the unit of measure of æther masses we have obviously taken here the mass of æther which is capable of giving to another mass of equal amount the acceleration 1 in the time 1, the distance between the masses being 1. If, on the contrary, m' only is at rest while m moves with the constant velocity h in a direction making the angle θ with the line of junction between the two molecules, we have, for the case in which m approaches m' (in which we designate the acute angle by θ),

$$-\frac{mm'}{r^2}\left[1+\phi(-h\cos\theta)+\psi\left(\frac{h^2}{r}\left[1-\cos^2\theta\right]\right)\right]$$

as the expression of the repulsion according to equation (1) of the first part of this memoir.

For the case in which m recedes from m', and designating the obtuse angle by θ, we obtain the same formula, except only that $h\cos\theta$ (which is equal to the projection of the velocity along the line of junction) has the opposite sign.

From equations (7) and (10) we have :—

$$\psi\left(\frac{h^2}{r}\left[1-\cos^2\theta\right]\right)=\frac{k}{2}h^2(1-\cos^2\theta),$$

and

$$\phi(-h\cos\theta)\qquad=-ah\cos\theta-\frac{k}{4}h^2\cos^2\theta.$$

Introducing these values of ψ and ϕ into the above expression

of the repulsion between two molecules of which only one is in motion, we obtain :—

$$-\frac{mm'}{r^2}\left[1 - ah\cos\theta + \frac{k}{2}h^2\left(1 - \frac{3}{2}\cos^2\theta\right)\right]. \quad . \quad (12)$$

If m is receding from m', the angle θ is obtuse and the second term becomes positive.

Formula (12) expresses the direct repulsion between m and m', the first being in motion and the second at rest. Now the molecule m is also repelled by all the rest of the mass of æther which surrounds it. At the first instant, before the molecules have been able to change their positions of equilibrium, the resultant of the repulsions exerted upon m' by the rest of the surrounding æther will be equal to the repulsion between m (considered at rest) and m', but will have an opposite direction. This comes out evidently from the fact that the resultant of the repulsions exerted upon m' by *all* the surrounding mass of æther was $=0$ when the molecule m was still at rest. We obtain, then, the sum of the forces which, at the *first* instant that m is put in motion, act upon the molecule m', if we subtract from the repulsion expressed by formula (12) the repulsion between m and m' when the former is at rest. It follows from this that at the *first* instant the molecule m' is repelled along the line of junction between m and m' with a force which is expressed by

$$+\frac{mm'}{r^2}\left[ah\cos\theta - \frac{k}{2}h^2\left(1 - \frac{3}{2}\cos^2\theta\right)\right]. \quad . \quad . \quad . \quad (13)$$

If this expression be negative, the molecule m' will tend to remove itself from m in the direction of their line of junction; if, on the contrary, it be positive, an approach will be effected along the same line. If m recedes from m', the angle θ is greater than a right angle, and consequently the first term is negative; if, on the contrary, an approach takes place, that term is positive.

If, then, μ designate the quantity of æther in motion in the unit of length of the conductor in which m moves, and ds be the element of that conductor, m will be equal to μds. Now μh is equal to the intensity i of the current. In an analogous manner m' may be replaced by $\mu' ds'$. We thus obtain instead of formula (13):—

$$+\frac{i\mu'}{r^2}\left[a\cos\theta - \frac{k}{2}h\left(1 - \frac{3}{2}\cos^2\theta\right)\right]ds\,ds'. \quad . \quad . \quad (14)$$

Formula (14) is the expression of the force with which an element of the inducing current whose intensity is i tends *at the first instant* to move, in the induced circuit, the quantity of æther $\mu' ds'$ along the line of junction between the two elements.

This is the maximum value of the force; after the first moment, it diminishes continually until, at last, it becomes $=0$ when the molecules have reached their new positions of equilibrium.

Formula (14) can be divided into two parts, viz.

$$+ \frac{i\mu'a}{r^2}\cos\theta \, ds \, ds', \text{ and } \frac{k}{2r^2}i\mu'h\left(1 - \frac{3}{2}\cos^2\theta\right)ds \, ds'.$$

If in the second part we denote by i' the intensity of current indicated by $\mu'h$, that part of the formula becomes

$$- \frac{kii'}{2r^2}\left(1 - \frac{3}{2}\cos^2\theta\right)ds \, ds'.$$

Now this expression indicates half of the electrodynamic repulsion between two circuit-elements ds and ds' when they are parallel and respectively traversed by the currents i and i'.

In the theoretic deduction of the electrodynamic formulæ, we have assumed that the repulsion between two molecules of æther is communicated without diminution even to the elements of the circuit in which they are moving. Of course this hypothesis applies only to the part of the repulsion-force between the molecules which remains in the electrodynamic formulæ, and not to the part which vanishes (of itself) in the formation of those formulæ. It consequently applied to the terms in the expression of the repulsion which are multiplied by k, and not to that containing the constant a. But if we maintain this hypothesis for the terms multiplied by k in the expression of the repulsion between two æther molecules, the theoretic deduction gives a result perfectly accordant with Ampère's empiric formula.

But the communication of the part in question of the repulsive force entirely to the circuit-elements in which the molecules move, presupposes necessarily that that part cannot communicate any proper motion to the molecules themselves in their respective circuits; for, if it did, a portion of the repulsion would be expended in generating that motion and in producing the heat resulting from the resistance opposed by the circuit. In this case, then, the whole of the repulsion could not pass to the circuit-elements. Still the molecules of æther may possibly receive some motion from the repulsion mentioned, but too slight to permit a difference between theory and experiment to be observed in electrodynamic phenomena. Be it as it may in this respect, we obtain, as a necessary consequence of the hypothesis we have made in the deduction of the electrodynamic phenomena, that the terms in formula (14) which are multiplied by the constant k exert only an insignificant influence on the displacement of the æther particles in the induced circuit, and that consequently their importance for the induction is very little.

But evidently, that this may be so, kh (or the velocity of the æther in the inducing circuit multiplied by the constant k) must have a very small numerical value. With regard to the velocity h, experiments have not, as we have seen, led to an accordant result. Fizeau and Gounelle found that the velocity rose in a copper wire to 180 millions, and in an iron wire to 100 millions of metres in a second. Walker estimated the velocity in an iron wire at 30 millions only, and Gould at less than 26 millions in a wire of the same metal. The experiments made on a copper telegraph-wire between Greenwich and Edinburgh gave a little more than 12 millions of metres per second; and only $4\frac{1}{3}$ millions was obtained on the telegraphic line which connects Greenwich with Brussels. The small velocity on this last line, which was also of copper, may be partly explained by a great length of it being submarine. It must further be remarked that, from the manner in which the experiments were made, the numbers cited express the velocity with which the first quantity of æther propagates itself, at the commencement of the current, from one pole to the other. The ratio of this velocity to the velocity when the current continues with constant intensity has not yet been determined by experiment. Of the velocity of the æther in a wire under the conditions of an ordinary induction experiment we know almost nothing more than that it is very great.

The constant k enters as a factor in Ampère's formula for electrodynamic phenomena; and its value has been determined experimentally by W. Weber and Kohlrausch[*]. Taking as unit that above given for the measure of the æther, we have from the experiments of those two physicists, in round numbers :—

$$\frac{1}{\sqrt{k}} = 440 \text{ millions of metres per second.}$$

Admitting for the velocity h a value within the limits of the results of the above-mentioned experiments, a very small value is obtained for the product kh. Now the constant a of the first term of formula (14) should also have only a trifling numerical value. It follows evidently, from the considerations set forth in these pages, that ah must be less than 1. If, then, we admit for example the value of h found by Walker, or 30 millions of metres per second, the value of a will be $< \dfrac{1}{30 \text{ millions}}$. On this supposition, the product $\dfrac{kh}{2}$ will be $= \dfrac{1}{12,900 \text{ millions}}$, and consequently a may be 400 times $\dfrac{kh}{2}$. This shows that, far from con-

* Pogg. *Ann.* vol. xcix. p. 10.

tradicting, experiment much rather confirms the result arrived at theoretically—namely, that the terms in formula (14) multiplied by $\frac{kh}{2}$ may be neglected in comparison with the first.   Experiment alone can decide if this is permitted in reality.

The action upon the æther of the circuit-element ds', expressed by formula (14), is exerted along the line of junction between ds and ds'.   But as the æther of ds' can only move along this element, we must, in order to have the measure of the motion produced in the æther of ds, multiply that expression by the cosine of the angle formed by the acting force with the circuit-element.   Calling this angle θ', we must multiply by $\cos \theta'$. By electromotive force of induction is meant the *accelerative* force exerted by the inducing wire upon the æther contained in unit of length of the induced wire.   The value of this we obtain by dividing the expression (14) by μ'; and thus we get as expression of the induction of a current-element upon an element of the induced circuit during the first instant :—

$$+ \frac{i}{r^2} \left[a \cos \theta - \frac{kh}{2} \left(1 - \frac{3}{2} \cos^2 \theta \right) \right] \cos \theta' \, ds \, ds'. \quad . \quad (15)$$

The induced circuit must always be closed that an induction-current may be possible.   In the integration of the formula (15) with respect to ds', the term independent of $\cos \theta$ (viz. $\frac{khi}{2r^2} \cos \theta' ds \, ds'$) vanishes, whatever be the form of the induced circuit, provided it is closed.   This is easily proved by the following reasoning. Imagine two spherical surfaces with the element ds for centre, the one having the radius r, and the other the radius $r + dr$. Now, if a part of the induced circuit be upon either of these concentric surfaces, evidently the above-mentioned term must vanish for that part of the circuit.   Everywhere, in this case, $\cos \theta$ is $= 0$, since the radius of a sphere always makes a right angle with the lines drawn from the terminal point of the radius upon the surface of the sphere.   The elements of the induced circuit which fall between the two concentric surfaces must be in pairs, since the circuit is closed.   A current, then, in the induced circuit will pass as often from the outer to the inner surface as from this to the former.   The cosine of the angle θ', which any one of the elements included between the surfaces forms with the corresponding radius, is equal to $\frac{dr}{ds}$; and the number of these cosines bearing a positive sign is equal to that of the cosines with a negative sign.   It hence follows that, for all the elements which fall between the two surfaces, the product

of $\dfrac{kih}{2r^2} ds \dfrac{dr}{ds'} ds'$ will be equal to zero. Now, as this is true for any value of r, it must in like manner be true for the whole circuit. We can therefore, in the place of formula (15), employ in the integration the formula

$$+ \frac{i}{r^2}\Big(a \cos \theta + \frac{3}{4} kh \cos^2\theta\Big) \cos \theta' \, ds \, ds'. \quad . \quad (16)$$

Now this formula expresses the induction of the first instant only, before the molecules both of the wire and of the surrounding medium have been able to quit their primitive positions of equilibrium. But the induction continues until the new positions of equilibrium are reached, when the inductive force becomes zero. The force of induction undergoes a continual diminution from the commencement to the end of the duration of the induction; and formula (15) only gives the maximum value during the first instant. The result should be, that at the commencement of their existence induction-currents appear very strong and then diminish in intensity—a fact which has been proved by experiment[*]. If, then, we wish to calculate the amount of an induction-current in given circumstances, we must take into account not only the maximum value of the induction at the first instant Δt, but also the sum of all the inductions during the whole of the time of induction. If for the sake of brevity we designate the maximum value of the induction exerted by a current-element upon an element of the induced circuit by $\Delta t \, Ar$ when the distance between the elements is r, we can express the induction which takes place during the immediately following instant by $\Delta t p Ar$, when p is less than unity. In this way the sum of all the inductions will be

$$\Delta t(1 + p + p_i + p_{ii} + \ldots + 0)Ar,$$

in which each consecutive term of the series is less than the preceding. This can be expressed more briefly by $\Delta t \mathrm{F} Ar$, where F denotes the sum of the series. For another element of the induced current, of which the distance from the inducing element is r_1, we obtain in the same way $\Delta t \mathrm{F}_1 Ar_1$. Now, if F were always equal to F_1 (that is, if the sum of the series were constant), the sum of the inductions would be proportional to the maximum value, whatever might be the variations in the force i of the current and in the distance r between the elements, and we could at once calculate from formula (16) the relative magnitude of the induction-current. The fact that F is independent of i cannot

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* See Lemström, *K. Vet.-Akademiens Handl.* ny följd. vol. viii. (1869); Blaserna, *Giornale di Scienze Naturali ed Economiche*, vol. vi. (Palermo, 1870).

be doubted; but the same cannot be said with respect to r. The force of induction at a given moment upon the mass of æther $\mu' ds'$ of the induced circuit is proportional to the difference between the repulsion exerted upon $\mu' ds'$ by the element of the inducing current (in which the mass of æther μds moves with the velocity h) and the repulsion upon the same mass of all the rest of the æther. The first of these repulsions diminishes (as is evident from the preceding reasoning) inversely as the square of the distance between the elements ds and ds'. Now, if this were also the case with the latter, viz. the repulsion exerted upon $\mu' ds'$ by all the rest of the mass of æther, F would evidently be independent of r; for we could express, for a given instant, by $\frac{a}{r^2}$ the repulsion proceeding from the element ds of the inducing current, and that of all the remaining mass of æther by $\frac{b}{r^2}$, expressions in which a and b would be constants. The force of induction for that moment would then become $\frac{1}{r^2}(a-b)$, which may be written pAr, p being a constant. As long as the æther molecules are in their normal primitive positions of equilibrium, the repulsion exerted upon $\mu' ds'$ by all the surrounding mass of æther with the exception of μds is $=+\frac{\mu \mu' ds\, ds'}{r^2}$, and therefore indeed diminishes inversely as the square of the distance. But this can no longer be the case after the molecules are displaced and the mass of æther about $\mu' ds'$ has undergone a distribution different from its normal condition; for the repulsion exerted upon $\mu' ds'$ by the surrounding æther is of course not independent of the distribution of the æther. F must necessarily depend on r; and hence we shall write $\mathrm{F}(r)$ instead of F.

We have therefore obtained the following formula to express the magnitude of the induction-current :—

$$+ \frac{i\,\mathrm{F}(r)}{r^2}\left(a \cos\theta + \frac{3}{4}kh \cos^2\theta\right)\cos\theta'\, ds\, ds', \quad . \quad (17)$$

or, if we neglect the last term,

$$+\frac{ai\,\mathrm{F}}{r^2}\cos\theta \cos\theta'\, ds\, ds'. \quad . \quad . \quad . \quad (18)$$

We now suppose that the inducing current is closed, and that its form is such that it can be divided by a plane into two symmetric halves. Then each element a on one side of the plane has a symmetrically corresponding element a' on the other side. We suppose further that the induced circuit is closed and symmetric about the same plane; so that to each element b on the

former side corresponds a symmetric element b' on the other. It thence follows that the distance between a and b' must be as great as that between a' and b, that the cosine of the angle between the element a and the line of junction ab' must be equal to the cosine of that between a' and $a'b$, but that the cosines will have opposite signs, since the directions of the elements on the two sides of the plane are determined by the direction of a current which traverses the circuit. In the same way, the cosines of the angles formed by the above-mentioned lines of junction and the elements b and b' of the induced circuit will be of equal magnitude but have contrary signs. Thus, in the induction of the element a upon b' and of a' upon b, the two cosines of θ will be equal to each other in magnitude, but will have contrary signs, which is also the case with the two cosines of θ'. It hence results that the part of the induction which corresponds to the term in formula (17) into which $\cos^2 \theta$ enters will be $= 0$ for the two symmetric elements combined. It will be the same with all the other symmetric elements. Consequently, if the two circuits, inducing and induced, be each cut symmetrically by one and the same plane, the integral of the term into which $\cos^2 \theta$ enters will be equal to zero. In this case, then, the integrals of the formulæ (17) and (18) will be perfectly equal.

We will now compare the theoretic result with the results of experiment.

We suppose that the circuits of both the inducing and the induced current are circular, that the radius of the first is R, and that of the second is R_1, that the planes of the two circles are parallel, and that the line joining the two centres makes a right angle with those planes. In this case the two circuits are placed symmetrically about the same plane, and the induction-formula (18) is applicable. Imagining then the inducing circle situated in the plane of xy of a system of rectangular coordinates of which the origin is at the centre of the circle, the induced circle is at a certain distance z from this plane. The distance r from an element ds, of which the coordinates are $x = 0$ and $y = -R$, an element situated in the inducing circle, to an element ds' with the coordinates x_1, y_1, z_1, situated in the induced circle, is then equal to $+\sqrt{x_1^2 + (y_1 + R)^2 + z_1^2}$, or, what comes to the same, to $+\sqrt{R_1^2 + R^2 + 2Ry_1 + z_1^2}$. The tangent of the element ds is parallel to the axis of the x's; and if we assume that the inducing current passes in the positive direction of the axis, $\cos \theta = \dfrac{x_1}{r}$ and consequently changes sign with x_1. If the element ds' of the induced current be reckoned in the direction

opposite to that of the inducing current, $\cos \theta'$ will be $= \dfrac{x_1 \mathrm{R}}{r \mathrm{R}_1}$, which also changes sign with x_1. Introducing these values of r, of $\cos \theta$, and $\cos \theta'$ into the induction-formula (18), we obtain

$$+ \frac{ai \, \mathrm{R} \, \mathrm{F}(r) x_1^2}{\mathrm{R}_1 r^4}.$$

It follows from this that the induction of the element ds is the same in both the halves into which the induced circuit is divided by the plane of yz, and that the induced currents go in the direction inverse to that of the inducing current on the same side.

But it is evident that each element of the inducing circle has the same inducing action as the element ds above considered. Therefore the total induction of the inducing circle upon an element of the induced circle will be

$$+ \frac{2\pi \mathrm{R}^2 ai \, \mathrm{F}(r) x_1^2}{\mathrm{R}_1 r^4} \, ds'.$$

Now $ds' = \dfrac{\mathrm{R}_1 dy_1}{\sqrt{\mathrm{R}_1^2 - y_1^2}}$; and $x_1^2 = \mathrm{R}_1^2 - y_1^2$. If we introduce these values and that of r, and (after taking the integral between the limits $y_1 = +\mathrm{R}$ and $y_1 = -\mathrm{R}$) multiply the latter by 2, we obtain as expression of the total induction, after replacing y_1 by $\mathrm{R}_1 u$ and consequently dy_1 by $\mathrm{R}_1 du$:—

$$+ 4\pi \mathrm{R}^2 \mathrm{R}_1^2 ai \int_{u=-1}^{u=+1} \frac{\mathrm{F}(r)\sqrt{1-u^2}\, du}{\left(\mathrm{R}_1^2 + \mathrm{R}^2 + 2\mathrm{R}\mathrm{R}_1 u + z_1^2\right)^2} \quad . \quad (19)$$

Felici has experimentally demonstrated the following principle. Let two circular current-circuits A and B, of equal radius R, be placed parallel at the distance z the one from the other, so that the line uniting their centres makes a right angle with the planes of the circuits; two other circular circuits C and D, each having the radius R_1, are placed in the same manner, but at a distance z_1 from each other such that $\dfrac{z}{\mathrm{R}} = \dfrac{z_1}{\mathrm{R}_1}$. If now through each of the circles A and C an inducing current of the same intensity be passed, the induced current of B will be to that of D as the radius R is to the radius R_1.

With the aid of this principle the function $\mathrm{F}(r)$ can be determined. If, in the above integral formula, R be made $= \mathrm{R}_1$ and $\mathrm{F}(r) = br = b\sqrt{2\mathrm{R}^2 + 2\mathrm{R}^2 u + z^2}$, in which b is a constant, we obtain

$$+ 4\pi abi\mathrm{R} \int_{u=-1}^{u=+1} \frac{\sqrt{1-u^2}\, du}{\left(2 + 2u + \dfrac{z^2}{\mathrm{R}^2}\right)^{\frac{3}{2}}}.$$

As what is under the sign of integration is independent of R, if $\frac{z}{R}$ remain constant, the induction-current will be proportional to R, in accordance with Felici's experiments.

In this way we obtain, instead of formula (18), the following expression of the induction between two elements :—

$$+\frac{abi}{r}\cos \theta \cos \theta' \, ds \, ds' \quad . \quad . \quad . \quad . \quad (20)$$

In order to ascertain if the results obtained by formula (19), after the determination of the function $F(r)$ in the way above indicated, agree with experiment, Dr. Sundell, Aggregate Professor at the University of Helsingfors, devoted himself to a great number of experiments at the physical laboratory of the Stockholm Royal Academy of Sciences. Such an inquiry was necessary to enable us rigorously to control the results obtained by theory; for previously we had only a very limited number of experiments applicable to the object we are here pursuing. We take the liberty of communicating one series of those experiments, referring the reader to Dr. Sundell's memoir itself for further details*.

The radius R of the induction-coil was equal to 21·7 centims.; the radius R_1 of the induced coil was 7·1 centims. The distance between the planes of the two circles is indicated in centims. under the letter z.

Deflections of the Magnetometer.

z	Observed.	Calculated.	Difference.
1·5	176·0	176·7	+0·7
10	127·4	128·3	+0·9
15	93·3	93·4	+0·1
20	66·1	66·0	−0·1
25	46·8	46·6	−0·2
30	33·8	33·3	−0·5
40	17·9	18·1	+0·2

The agreement between the calculations, on the one hand, and Dr. Sundell's experiments, on the other, is fully satisfactory in every respect.

If the inducing circle is in the plane of xy with the centre at the origin, and the induced circle in the plane of yz, but has its centre neither on the axis of z nor on the axis of y, integration causes the term of the induction-formula (17) containing $\cos \theta$ to vanish, while the other term, which contains $\cos \theta^2$, alone remains. Such an arrangement of the induction-circuits is consequently suitable for investigating whether that term has or has not an appreciable inductive force. By means of this pro-

* *Œfversigt af Vet.-Akad. Förh.* February 1872.

cedure Dr. Sundell has made some experiments, which have given no certain appreciable results ; and this likewise corroborates the theoretic deduction explained above.

The true law of induction between two elements is therefore expressed by the above-given formula (20). In virtue of the principles on which our present theoretic investigations are founded, it is evident that the formula in question applies also to the case in which induction takes place with a current of constant intensity, resulting from the distance between the inducing element ds and the induced element ds' diminishing from infinity to r.

5. *Distribution of the free æther at rest upon the conducting wire between the two poles of a battery.*—When a conducting wire with considerable resistance connects the poles of a galvanic battery, there is produced, as we know, free electricity at the surface of the wire. The positive electricity exhibits its maximum tension in the vicinity of the positive pole. With increasing distance from this the positive electricity diminishes; and if the resistance of the wire is the same in every part of its length, there exists in the centre an indifferent point, beyond which the second half of the wire shows itself negatively electric, with a tension which increases towards the negative pole. When the resistance of the wire is greater towards one extremity than towards the other, the indifferent point is nearer the same extremity as the greatest resistance. The difference between the electrical tensions at two points in the wire, divided by their mean resistance to conduction, is everywhere constant. This position of equilibrium of free electricity appears difficult to explain ; for it seems that the negative and positive electricities ought to clear the indifferent point and combine. Nor in this respect has any explanation been hitherto given satisfactory and free from all arbitrary hypothesis. The theory now presented offers, as of itself, that explanation :—When a galvanic current commences, the molecules of the surrounding mass of æther abandon the positions of equilibrium which they have hitherto occupied, and pass into new positions ; hence there results an induced current in a neighbouring closed conductor. The molecules which are in a nonconducting body near are also driven from their positions of equilibrium, and take new ones, although the absence of conductivity prevents the rise of an induction-current properly so called. The molecules remain in their new positions of equilibrium as long as the active cause (the galvanic current) continues with constant force. The law of the action of an element of the inducing current upon an element of the induced current is expressed by the formulæ given above. But it is evident that it must be altogether the same

with two elements ds and ds' in one and the same closed circuit. The galvanic current, then, tends from the commencement to produce a current in a direction opposite to its own. The electromotive force of the pile obstructs this motion.

The æther of the conducting wire which unites the two poles is carried by the force of induction to the positive pole, and there collects until its tension is sufficient to overcome the resistance opposed by the electromotive force or to surmount the inductive force. It is perfectly evident that the density of the æther must diminish as the distance from the positive pole increases. The quantity of æther contained in the wire being constant, when that æther is conducted towards the positive pole there must result a deficit of æther at the negative pole; and this deficit will be equal to the excess at the positive pole. A direct consequence of the preceding considerations is that the algebraic difference between the excess and the deficit must be proportional to the intensity of the current.

6. *The Chemical and other related phenomena.*—The limits of this memoir do not permit us to give here a complete exposition in detail of the application of the above-mentioned theory to the action of the galvanic current. We can only trace the starting-points of the explanation of the chemical phenomena. We will first call attention to the fact that the theory of induction given in the preceding pages has placed at our disposal a new force in permanent activity as long as the current continues. This force, the magnitude of which is determined by formula (16), tends to carry a molecule of æther, previously at rest, in a direction opposite to that of the current itself. Suppose now the current traversing an electrolytic liquid constituting a chemical combination of two elements p and q, and that, according to the ordinary idea adopted by Berzelius and other chemists, p is electropositive, and q electronegative—that is to say (according to our view), that p has an excess and q a deficit of æther. It follows that the molecule p is carried by the current towards the positive pole with a greater force than the molecule q. As this action is effected in every part of the liquid, the latter molecule will even, in pursuance of the Archimedean principle, endeavour to arrive at the negative pole. If now the force with which the molecules tend in this way to move in opposite directions be greater than their chemical affinity, decomposition will result, and we shall have an excess of the molecules p at the positive pole, and of the molecules q at the negative pole.

In the first part of this memoir we expressed the opinion that the material particles of a liquid can be mechanically carried along with the current, and that in this fact may be seen the

chief cause of the phenomena studied by Wiedemann. But regard must also be had to the current-force expressed by formula (16), in virtue of which force the current tends to cause the æther molecules at rest to move in a direction opposite to its own. Now, if these molecules are intimately united with material particles, these latter must be carried along in the same direction. It is possible, therefore, to obtain for the particles in a liquid traversed by a galvanic current a motion in the one direction as well as in the other, since the direction depends upon which force presents the greatest intensity. We think that the phenomena of this category studied by Quincke* may be explained in this way, without having recourse to the action of the free electricity which is found at the surface of the liquid.

The circumstance that particles from the negative pole of a voltaic arc are carried to the positive pole, although in considerably less quantity than that of the particles which are detached by the current and impelled in the opposite direction, it must also be possible to attribute to the inductive force of the current, understood according to the theory here given.

7. *Rotation of the plane of polarization of light under the action of the current.*—In order to explain this phenomenon, it has generally been supposed that the material molecules of the transparent body in which the rotation is effected undergo a direct action from the galvanic current, and that this action produces the rotation of the plane of polarization. C. Neumann, on the contrary, considers that the rotation results from the action exerted upon the æther molecules by Ampère's molecular currents, which are due to the action of the galvanic current. He endeavours to demonstrate that the phenomena in question can be explained by the hypothesis that those molecular currents act upon the æther molecules as if these were electric. The preceding statement upon the nature of electricity shows that, of the two opinions, Neumann's approaches nearest to the truth. The æther of the transparent body round which the galvanic current passes cannot, under the action of the current, be in the normal state. The æther molecules have changed their positions of equilibrium; and molecular currents of æther are established, or, if they previously existed, have received a determined direction under the influence of the galvanic current. Neumann's opinion relative to the direct action of the molecular currents upon the molecules of æther is no longer an *hypothesis* requiring confirmation, but a *truth*, if it be true that the phenomena of electricity take place in the æther. Certainly, however, in this explanation we must also have regard to the change in the positions of equilibrium of the æther particles.

* Pogg. *Ann.* vol. cxiii. p. 513.

XXIV. *On the Hydrodynamical Theory of Attractive and Repulsive Forces.* By Professor Challis, *M.A., LL.D., F.R.S.**

AT the commencement of the "New Discussion of the Hydrodynamical Theory of Magnetism" contained in the Number of the Philosophical Magazine for June, I have enunciated the following principles :—(1) That all the *active* forces in nature are different modes of pressure under different circumstances of a universal elastic æther, which may be mathematically treated as a continuous substance pressing always proportionally to its density. (2) That all visible and tangible bodies consist of inert spherical atoms of constant magnitudes, held, when undisturbed, in positions of equilibrium by attractive and repulsive forces, acting according to laws which are referable both to the *active pressure* of the æther and the *passive resistance* of the atoms to such pressure due to the constancy of their form and magnitude. In conformity with these principles the æther at rest is assumed in that theory to be everywhere of the same density; and I have, besides, supposed the atoms to be so small that even in dense bodies the space they occupy is very small compared with the intervening spaces.

These physical principles (which are applied in the above-cited article in the theoretical explanation of a large number of facts both of galvanism and magnetism) will be seen to be partly *hypothetical* and partly *inferential*. All that relates to the qualities of the æther and the atoms is hypothetical; but what is asserted respecting the dependence of the laws of the physical forces on modes of pressure of the æther must rest on deductions by mathematical reasoning from the hypotheses, and can be established by nothing but such reasoning. To account by this deductive process for the *modus operandi* of forces to which facts of observation are attributed, has been the object of various articles which from time to time I have communicated to the Philosophical Magazine. After giving reconsideration to these theories, and in particular the Hydrodynamical Theory of Magnetism in the June Number, it occurred to me that they might be much elucidated by separately exhibiting the principles and processes of the mathematical reasoning which connects the different forces with hydrodynamical pressure; and accordingly to do this is the purpose of the present communication. It is to be understood that the following discussion has reference to reasoning founded on hypotheses, irrespectively of their being true or false, and that I do not ask any one to accept the hypotheses, but only to give consideration to the mathematical reasoning by which the consequences that result from them are ascertained. It may,

* Communicated by the Author.

however, be said that if the reasoning be such as cannot be called in question, it may possibly suffice to determine, by the consequences to which it leads, whether or not the hypotheses are true.

1. As it is proposed to account for the physical forces and the laws of their operation by hydrodynamical pressure, it will first of all be necessary to discuss the principles and rules of the application of mathematics in hydrodynamics. A perfect fluid (and such, by hypothesis, the æther is) may be defined to be one the elementary parts of which possess the properties (1) of being susceptible of movements which continually change their relative positions, (2) of being separable one from another without assignable force by the insertion of an indefinitely thin solid partition, (3) of pressing against each other and against any solid substance with which they are in contact. In order that the motions of the fluid and its pressures may be capable of mathematical treatment, the following axioms must be conceded :—first, that the directions of the motion in any elementary portion of the fluid are always and everywhere normals to a surface which is geometrically continuous through either a finite or an infinitely small extent; secondly, that the motions are consistent with the principle of constancy of mass; thirdly, that the pressures of the fluid, together with the action of extraneous forces, are governed by D'Alembert's Principle. These three axioms being granted, mathematical reasoning founded upon them leads to general differential equations, from the integrals of which may be determined the motion and pressure of the fluid under given circumstances.

2. The first axiom has reference to the principle of geometrical continuity, to which, it is clear, the motions of the fluid must be subject. Calling, for the sake of shortness, the surface to which the directions of the motion are normal " a surface of displacement," it is regarded as an axiom that for each element there is in successive instants such a surface. It is, however, to be considered that a surface of displacement of finite extent may consist of an unlimited number of parts the equations of which are expressed by different functions, but that neither the tangent-planes of two contiguous parts at a given instant, nor the tangent-planes of the surfaces of displacement of a *given* particle in successive instants, can make a finite angle with each other. These conditions of continuity, which are dynamical rather than geometrical, exclude changes *per saltum* of the directions of motion with respect both to space and time, forasmuch as such changes could only be effected by *infinite* forces.

3. Let, therefore, u, v, w be the velocities, resolved in the directions of the axes of coordinates, at the point of a surface of

displacement the coordinates of which at the time t are x, y, z. Then, passing from that point to any other indefinitely near on the same surface, the coordinates of which are $x+dx$, $y+dy$, $z+dz$, the equation

$$\frac{u}{\lambda}\,dx + \frac{v}{\lambda}\,dy + \frac{w}{\lambda}\,dz = 0$$

will express generally that the motion at each point is in the direction of a normal to the surface, $\frac{1}{\lambda}$ being a factor which makes the left-hand side of this equation a complete differential. Hence, representing that differential by $(d\psi)$, according to the above-stated axiom we shall have, as well as $(d\psi)=0$, also $\delta(d\psi)=0$, the symbol δ having reference to change of the surface of displacement of the given element by change of the time and of its position. On account of the independence of the symbols of operation δ and d, that equation is equivalent to $(d\,.\,\delta\psi)=0$. But

$$\delta\psi = \frac{d\psi}{dt}\,\delta t + \frac{d\psi}{dx}\,\delta x + \frac{d\psi}{dy}\,\delta y + \frac{d\psi}{dz}\,\delta z\,;$$

and because the variation with respect to time has reference to the change of position of the given element,

$$\delta x = u\delta t, \quad \delta y = v\delta t, \quad \delta z = w\delta t.$$

Hence

$$(d\,.\,\delta\psi) = \left(d\,.\,\left[\frac{d\psi}{dt} + \frac{d\psi}{dx}\,u + \frac{d\psi}{dy}\,v + \frac{d\psi}{dz}\,w\right]\delta t\right) = 0.$$

Consequently the differential, with respect to space, of the quantity within the brackets [ ] is zero, and by integration

$$\frac{d\psi}{dt} + \frac{d\psi}{dx}\,u + \frac{d\psi}{dy}\,v + \frac{d\psi}{dz}\,w = \chi(t). \quad . \quad . \quad . \quad . \quad \text{(a)}$$

4. The reasoning thus far has already been given under the head of Proposition VI. in an article "On the Principles of Hydrodynamics" in the Philosophical Magazine for January 1851, and more recently in pp. 174 and 175 of the 'Principles of Mathematics and Physics.' It is reproduced here for the purpose of drawing an important inference which I had previously overlooked. Before doing so, however, it will be proper to enunciate for future use the following general rule respecting the application of analysis to physical questions. When the fundamental principles of any department of applied mathematics have been expressed in the form of differential equations, the solutions of the equations are coextensive with the physical consequences of the principles; so that there is no such consequence which the solved equations do not embrace, and no positive

analytical result of the solutions which does not correspond to a physical reality. This rule is applied in the following inferences from the general equation (a).

5. That equation is plainly equivalent to $\left(\dfrac{d\psi}{dt}\right)=\chi(t)$; whence by integration $\psi=\chi_{\prime}(t)+C$, $\chi_{\prime}(t)$ being an arbitrary function of the time, and C an arbitrary quantity independent of t. The argument in art. 17 of the communication in the June Number, being conducted so that $\chi(t)$ is included in the function ψ, gives $\left(\dfrac{d\psi}{dt}\right)=0$, and by integration $\psi=C$, an arbitrary quantity not containing t. The present argument shows that the function ψ has in fact this character if $\chi(t)$ be assumed to be zero, but that it may also vary in an arbitrary manner with the time. Now, according to the foregoing rule, this result must be taken into account as well as the other, both being significant; and the inference to be drawn is that, consistently with the principle of geometrical continuity, there may be *two* classes of motions, for one of which the functions ψ have constant values and the surfaces of displacement have fixed positions in space, and for the other these functions vary with the time, and the surfaces of displacement are continually shifting their positions. Generally, the former is the class of *steady* motions, and the other that of *unsteady* or *vibratory* motions. It should be observed that the actuality of such motions cannot be demonstrated without taking account of the other general equations, and that the foregoing reasoning only shows that their existence would be compatible with the principle of geometrical continuity.

6. To proceed in logical order, it would next be required to investigate mathematically the general differential equation

$$\frac{d\rho}{dt}+\frac{d\cdot\rho u}{dx}+\frac{d\cdot\rho v}{dy}+\frac{d\cdot\rho w}{dz}=0, \quad . \quad . \quad . \quad (b)$$

which is derived from the principle of constancy of mass, and the three dynamical equations,

$$\frac{dp}{\rho dx}=X-\left(\frac{du}{dt}\right), \qquad \frac{dp}{\rho dy}=Y-\left(\frac{dv}{dt}\right), \qquad \frac{dp}{\rho dz}=Z-\left(\frac{dw}{dt}\right), \text{ (c)}$$

which are given by D'Alembert's Principle; but these investigations are so well known that they need not be introduced here.

N.B. The expression $udx+vdy+wdz$ occurs so frequently that in future it will be designated by $[udx]$.

7. The reasoning thus far would be the same whether we supposed $[udx]$ to be an exact differential, or to become such by

means of the factor $\frac{1}{\lambda}$. As there is an analytical distinction be-
tween these two cases, according to the before-cited rule there
must exist, consistently with the principle of geometrical conti-
nuity, a corresponding difference in the actual circumstances of
the motion. It is possible to point out such difference by em-
ploying the following argument. In case $[udx]$ be an exact
differential, the equation $[udx] = 0$ is the differential equation of
a surface the curvature of which is generally finite, and conti-
nuous through at least an indefinitely small extent. Hence it
follows, since the normals converge to two focal lines, that the
form of the element to which the coordinates x, y, z apply at the
time t is undergoing change. The same is the case with respect
to any other element, the continual change of form of identical
portions of matter being a general characteristic of a fluid mass
in motion. But it is also possible that a mass of fluid may
under certain circumstances move in such manner that each ele-
ment continues to be of the same form throughout the motion—
for instance, if the fluid rotate about a fixed axis, and the velocity
be a function of the distance from the axis. For by the prin-
ciple of easy divisibility the fluid may in that case be conceived
to be divided into indefinitely thin cylindrical shells, having as
their common axis the axis of the motion, the velocity of rota-
tion of any shell being at the same time a function of the dis-
tance from the axis. Also on the same principle each shell
might have, in addition, a motion of translation parallel to the
axis, the form of every element of the shell still remaining
constant.

8. In articles 18 to 25 of the communication in the June
Number I have shown that for the above-mentioned cases of con-
stancy of form of the elements in motion, $[udx]$ is integrable by
a factor, and that this analytical circumstance not only proves
that such motions are compatible with the principle of geome-
trical continuity, but also distinguishes them from all other mo-
tions. I have also in the same articles determined the condi-
tions under which these motions satisfy the general equations
(b) and (c), whence it appears that they must be *steady* motions.
I consider it therefore unnecessary to introduce these investiga-
tions here, and shall only remark further on this part of the
subject, that the cases of motion for which $[udx]$ is integrable
by a factor are applied in the hydrodynamical theory of *galvanic
currents along slender wires.* (See arts. 26–40 in the Theory of
Galvanism and Magnetism, contained in the June Number.) I
proceed now to the discussion of the classes of motion for which
$[udx]$ is integrable *per se.*

9. Resuming the equation (a), and putting it under the form

$$\frac{d\psi}{dt} + \frac{(d\psi)}{dt} = \chi(t),$$

there will be two cases for consideration : first, that for which ψ is independent of t, in which case $[udx]$ may, from what is said in the preceding paragraph, either be an exact differential, or be integrable by a factor; and secondly, that in which ψ varies with the time and $[udx]$ is necessarily an exact differential.

10. Taking the case in which ψ does not contain t, we have $\frac{d\psi}{dt} = 0$, so that $(d\psi) = \chi(t)\,dt$. In order that this result may be consistent with the condition that ψ is independent of t, we must have $\chi(t) = C$, an arbitrary constant; and then, since dt may be considered constant, it will follow that $(d\psi)$, which is the change of ψ in passing at a given instant from any point to a contiguous point, is an *arbitrary* infinitesimal quantity. Being of arbitrary value we may suppose it to vanish, or that $(d\psi) = 0$. Since this is the differential equation of a surface of displacement, the preceding argument has shown that such a surface can exist, consistently with the principle of geometrical continuity, in every case of steady motion, whether $[udx]$ be an exact differential or be integrable by a factor.

11. It will be now proper to state that in the proposed hydrodynamical theory of the physical forces, certain of these forces are referable to pressure of the æther in steady motion, while the remainder are accounted for by pressure accompanying its unsteady or vibratory motions. Two classes of forces, known experimentally, are found to correspond to two kinds of motion of a fluid which have been ascertained by the aid of mathematics. The forces which correspond to *steady* motions of the æther will first be considered.

12. The way in which the particular cases of steady motion for which $[udx]$ is *integrable by a factor* account for the conduction of *galvanic currents* along slender wires has already been sufficiently referred to in art. 8 of this communication, and need not be discussed here.

13. The forces which, according to the theory, are ascribable to pressures of the æther in the cases of steady motion for which $[udx]$ is an *exact differential*, are *magnetic, galvanic,* and *electric attractions* and *repulsions*. It is assumed to be a necessary condition of the existence and maintenance of the appropriate steady motions, that a gradation of atomic density should exist in the interiors of the attracting or repelling substances. In arts. 4 to 9 of the Theory of Magnetism in the June Number I have pro-

posed a mathematical theory of the generation of steady streams under that condition.

14. Respecting the *origination* of the gradations of atomic density, it will suffice for my present purpose to say that in galvanism the producing cause appears to be chemical action between dissimilar substances in contact; in magnetism the act of magnetizing may be supposed to generate a gradation of atomic density which is afterwards maintained, with more or less persistence, by the intrinsic atomic and molecular forces of the magnetized body; and in frictional electricity the friction seems to superinduce an abnormal state of equilibrium of the atoms of an extremely thin superficial stratum of the electrified substance, together with an interior gradation of its atomic density, depending, as to degree and permanence, on the capacity of its intrinsic atomic and molecular forces to retain the superficial atoms in the abnormal positions.

15. Now, in whatever way steady streams are generated, according to hydrodynamics they will be accompanied by variations of the density and velocity of the fluid from point to point of space, while the density and velocity at any given point and the direction of the velocity will be constant. Hence it evidently follows that a small spherical atom, immersed in the æther under these circumstances, will be differently pressed at different points owing both to the motion and to the variation of density of the fluid, and consequently that the atom will in general be acted upon by an accelerative force. The exact mode of this action is now to be investigated. The following mathematical reasoning employed for this purpose is almost exactly the same as that I have given in the solution of Example VIII. in p. 313 of the 'Principles of Mathematics and Physics.' But as that reasoning is not directly referred to in the Theory of Magnetism in the June Number, and I have reason to think that on account of this omission some difficulty may be felt in following the arguments, I propose to reproduce it here.

16. The investigation being restricted to motion for which $[udx]$ is an exact differential, the equations (b) and (c) give, by the usual process,

$$a^2 \, \text{Nap.} \log \rho + \frac{d\phi}{dt} + \frac{V^2}{2} + f(t) = 0;$$

and as the motion is steady, $\dfrac{d\phi}{dt}$, which is equal to $\displaystyle\int \frac{dV}{dt} \, ds$, vanishes. Since also the fluid is by hypothesis of unlimited extent, there will be distant points at which $V = 0$ and the density ρ is that which the fluid has in its undisturbed state. Calling this

density ρ_0, we shall have a^2 Nap. $\log \rho_0 + f(t) = 0$.   Consequently

$$\rho = \rho_0 e^{-\frac{V^2}{2a^2}}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (d)$$

which is an exact equation, applying to the fluid at every point of space it occupies, provided it is acted upon by no extraneous force.   It will be seen that I have not here substituted the constant a' for a, as in art. 8 of the Theory of Magnetism, the reason being that, according to the hydrodynamical principles which I have long maintained, the substitution is not required when the motion is steady.

17. Respecting the above expression for ρ, it is to be observed that it not only applies to the *whole* of the fluid in steady motion, but applies also whether the steady motion be simple or be compounded of two or more steady motions.   For, according to hydrodynamics, such motions may *coexist*, and the resultant is consequently steady motion.   (See ' Principles of Mathematics and Physics,' pp. 242 & 243.)   As V will always be very small compared with a, instead of equation (d) we may use

$$\rho = \rho_0 \left(1 - \frac{V^2}{2a^2} \right).$$

18. Let us conceive, at first, the spherical atom to be fixed, and to be acted upon by a stream of the æther in steady motion. Then if the lines of motion were parallel, the distribution of density on the surface of the sphere, due to the sphere's reaction, would be symmetrical with respect to a plane through its centre perpendicular to the direction of the stream.   (For proofs of this proposition I may refer to an article in the Philosophical Magazine for November 1859 (p. 323), and to the argument concluded in p. 302 of the ' Principles of Mathematics &c.')   Considering that the atom is of extremely small dimensions, and consequently that the lines of motion in the very slender portion of fluid incident upon it must originally be very nearly parallel, it may be admitted that the above-mentioned symmetrical distribution of density will not be sensibly affected by their convergency or divergency.   Thus, so far as regards any modification of the pressure arising from the reaction of the atom, we may suppose that no *accelerative* action upon the atom is thereby produced.   There remains only the accelerative force resulting from that variation of pressure from point to point of the spherical surface, which is due exclusively to the steady motion, and would be sensibly the same at the same points if the motion were not interfered with by the reaction of the atom.

19. Suppose now that trajectory to *the surfaces of equal pressure* to be drawn the direction of which passes through the

centre of the sphere, and let s be any length along this line reckoned from a given point on it as origin. The sphere being by hypothesis extremely small, it may be assumed, in accordance with what is argued above, that for all points of any transverse circular area the centre of which is on the trajectory, and the radius not less than the radius of the sphere, with sufficient approximation $\rho = f(s)$. Let s_i be the value of s corresponding to the sphere's centre, and let θ be the angle which any radius of the sphere makes with the trajectory, so reckoned that for a point of the surface $s = s_i - b \cos \theta$, b being the radius. Then for any such point

$$\rho = f(s_i - b \cos \theta) = f(s_i) - b \cos \theta f'(s_i) \text{ very nearly,}$$

the remaining terms being omitted because the variation of ρ through the small extent of the sphere's diameter may be assumed, with sufficient approximation, to be uniform. Accordingly the whole pressure on the sphere estimated in the positive direction of s is

$$2\pi \int a^2 \rho b^2 \sin \theta \cos \theta \, d\theta, \text{ from } \theta = 0 \text{ to } \theta = \pi.$$

This integral, on substituting the above value of ρ, will be found to be

$$-\frac{4\pi b^3 a^2}{3} f'(s_i).$$

Hence, Δ being the density of the sphere, the accelerative force is $-\dfrac{a^2}{\Delta} f'(s_i)$. If ρ_i and V_i be the density and velocity corresponding to the centre of the sphere,

$$\rho_i = f(s_i) = \rho_0 \left(1 - \frac{V_i^2}{2a^2}\right), \text{ and } f'(s_i) = -\frac{\rho_0}{a^2} \cdot \frac{V_i dV_i}{ds_i} .$$

Hence, by substituting for $f'(s_i)$, the expression for the accelerative force becomes

$$\frac{\rho_0}{\Delta} \cdot \frac{V_i dV_i}{ds_i} .$$

This result proves that the accelerative action on the fixed sphere has a constant ratio to the acceleration of the fluid where the sphere is situated, and is in the same direction. The direction is therefore *positive* or *negative* according as ρ *decreases* or *increases*, or according as V_i *increases* or *decreases* as s increases.

20. If the sphere, instead of being fixed, moved *uniformly* in a given direction, the accelerative action of the fluid upon it in any position would still have the same constant ratio to the acceleration of the fluid in the same position. For if the uniform motion be impressed in the contrary direction both on the fluid and the sphere, the latter will be reduced to rest, and the rela-

tive action will not be altered; and as under these circumstances a uniform stream is incident on the sphere at rest, from what has already been said in art. 18, no change in the accelerative action of the steady stream on the sphere is thereby produced.

21. If, however, the sphere be free to move in obedience to the pressure of a steady stream, it will in general be continually *accelerated,* and the acceleration will give rise to resistance due to the inertia of the surrounding fluid, which will in turn be impelled by the acceleration of the sphere. The amount of this retardation may be calculated just as in Poisson's solution of the resistance of the air to a ball-pendulum. Hence it follows, if $\frac{d^2 s_l}{dt^2}$ be the actual acceleration of the sphere, that the retarding force is $-\frac{\rho_0}{2\Delta} \cdot \frac{d^2 s_l}{dt^2}$ (Principles &c. p. 266), and consequently that

$$\frac{d^2 s_l}{dt^2} = \frac{\rho_0}{\Delta} \cdot \frac{V_l dV_l}{ds_l} - \frac{\rho_0}{2\Delta} \cdot \frac{d^2 s_l}{dt^2}.$$

Therefore

$$\frac{d^2 s_l}{dt^2} = \frac{2\rho_0}{2\Delta + \rho_0} \cdot \frac{V_l dV_l}{ds_l}.$$

It thus appears that, whether the atom be fixed or moveable, its acceleration by the pressure of the fluid in steady motion has a constant ratio to the acceleration of the fluid where the atom is situated. It may be observed that the acceleration is independent of the *magnitude* of the atom.

22. This result furnishes a general rule for ascertaining the mode and the direction of the motive action of magnetic and galvanic currents. It is required for that purpose to deduce from the given circumstances under which the steady motion, whether simple or composite, is produced, the values of ρ and V expressed as functions of coordinates, and also the courses of the trajectories to the surfaces of equal pressure. When by means of these deductions the accelerative force of the fluid is determined at each point as to magnitude and direction, in virtue of the foregoing equation both the magnitude and direction of the accelerative action on any atom are also determined. In illustration of this conclusion it may be added that by the above-mentioned trajectories the theory accounts for *the lines of magnetic force,* the courses of which are exhibited by the distribution and arrangement of iron filings submitted to the attraction of a magnetized bar.

It may now be stated that the many theoretical explanations of magnetic and galvanic phenomena given in the article in the June Number all depend on principles the consequences of

which, as deduced mathematically, have thus far been discussed; and I hope that this supplementary treatment of the *à priori* arguments may serve to make the explanations more intelligible and more worthy of acceptance. I proceed to the dynamical action of the *unsteady* motions of the æther.

23. This part of the inquiry is distinguished from that which precedes by the circumstance that at each point the velocity and its direction both vary with the time, or, if not both, that the velocity varies. (In the 'Principles of Mathematics,' p. 218, an instance of composite vibratory motion occurs in which the direction of the motion is constant.) Hence in the subsequent application of the general equation (a) it is assumed that the motion is not steady, and that consequently $[udx]$ is always and everywhere an exact differential (see art. 7). Accordingly, as ψ may vary with t, we may now suppose the arbitrary function $\chi(t)$ in that equation to be included in ψ; and then putting respectively $\lambda \dfrac{d\psi}{dx}$, $\lambda \dfrac{d\psi}{dy}$, $\lambda \dfrac{d\psi}{dz}$ for u, v, w, we have

$$\frac{d\psi}{dt} + \lambda \left(\frac{d\psi^2}{dx^2} + \frac{d\psi^2}{dy^2} + \frac{d\psi^2}{dz^2} \right) = 0. \quad . \quad . \quad . \quad (e)$$

An important inference is next to be drawn from this equation antecedently to making use of the other general equations. I have already given the reasoning proper for this purpose under Proposition VII. in the Philosophical Magazine for March 1851, and in pp. 186–188 of the before-cited volume. It is repeated here with certain modifications, which, I think, will have the effect of exhibiting more completely the logic of the argument.

24. Since $\lambda(d\psi) = [udx]$, and by hypothesis the right-hand side of this equality is an exact differential, it follows that λ is a function of ψ and t. Conceive a line to be drawn at a given instant in the directions of the motions of the particles through which it passes, and let x, y, z be the coordinates of a point the distance of which measured at the given instant along that line from a given point of the same is s. Then, V being the velocity at the point xyz,

$$\frac{(d\psi)}{ds} = \frac{d\psi}{dx} \cdot \frac{dx}{ds} + \frac{d\psi}{dy} \cdot \frac{dy}{ds} + \frac{d\psi}{dz} \cdot \frac{dz}{ds}$$

$$= \frac{d\psi}{dx} \cdot \frac{u}{V} + \frac{d\psi}{dy} \cdot \frac{v}{V} + \frac{d\psi}{dz} \cdot \frac{w}{V}$$

$$= \frac{1}{\lambda V} (u^2 + v^2 + w^2) = \frac{V}{\lambda}.$$

But by the equation (e),

$$\frac{d\psi}{dt} + \frac{V^2}{\lambda} = 0.$$

Hence, substituting $\lambda \dfrac{d\psi}{ds}$ for V,

$$\frac{d\psi}{dt} + \lambda \frac{d\psi^2}{ds^2} = 0.$$

Introducing now the condition that λ is a function of ψ and t, this equation must admit of an integral of the form $\psi = f(s, t)$, in which s is, by definition, the length of a trajectory of surfaces of displacement.   Now we may suppose that at the time t one extremity of the trajectory is on a certain *given* surface, and the other on the undetermined surface which passes through the point whose coordinates at the time t are x, y, z.   Accordingly the above value of ψ may be taken to be *general* as to space for the time t, so that its variation with respect to space gives for *any* position :—

$$(\delta\psi) = \frac{d \cdot f(s, t)}{ds}(\delta s).$$

25. Since $(\delta\psi)$ is the *variation* of the function ψ in passing at the given time from the point xyz to *any* contiguous point, we may inquire, as in the Calculus of Variations, under what conditions that function may have a maximum or minimum value. Let, therefore, $(\delta\psi) = 0$. Then, since $\dfrac{d \cdot f(s, t)}{ds}$, which is equal to $\dfrac{d\psi}{ds}$ or $\dfrac{V}{\lambda}$, may be supposed not to vanish, the sole condition is that $(\delta s) = 0$.   This result signifies that in the case of maximum or minimum, contiguous trajectories, intercepted between two surfaces of displacement separated by an arbitrary interval, are of the same length.   But this cannot be the case unless the trajectories are straight lines and the motion consequently rectilinear.   We have thus been led to an indication of rectilinear motion by arguing solely from the general equation (e), which was derived from the principle of geometrical continuity.   It is to be observed that this inference has been drawn antecedently to any supposition as to the mode of putting the fluid in motion, and that it rests on the abstract assumption that there is a certain form of the function ψ which gives it a maximum or minimum value.   Hence, if it be urged that the indicated rectilinear motion is such as might take place if the velocity were a function of the distance from a centre or from a fixed plane, the reply is that this interpretation is inadmissible be-

cause these rectilinear motions are produced under *arbitrary* conditions. Besides, it does not appear that for such motions ψ has the character of a maximum or minimum. The validity of the foregoing argument is confirmed by the fact, which I think I have on previous occasions sufficiently certified, that the two cases of rectilinear motion just mentioned cannot be mathematically treated with success till the circumstances and laws of the rectilinear motion now under consideration have been ascertained.

26. It is further to be noticed that, as the indication of rectilinear motion was arrived at by employing one general equation without reference to the other two, the result is not of general application, and we are not compelled to infer from it that the motion is always and everywhere rectilinear. Yet, according to the rule already laid down, the indication must be significant, and cannot without error be left out of consideration on proceeding to draw inferences from the other two equations. Accordingly I propose the following course of reasoning.

As the motion is not exclusively rectilinear, let it be supposed to take place in part along a straight line which relatively to the density and the rest of the motion is an *axis*. This hypothesis may be analytically expressed by first supposing the axis of the motion to coincide with the axis of z, and then assuming that

$$(d\psi) = (d \cdot f\phi) = u\,dx + v\,dy + w\,dz,$$

f being a function of x and y only, and ϕ a function of z and t only. The complete exhibition of the present argument would require to be given next an investigation of the consequences to which this supposition leads when the second and third general equations are taken into account. As this investigation is too long for insertion here, I can only refer to it as given in the Philosophical Magazine for August 1862, and in fuller detail in the proofs of Propositions XI. to XVII. contained in pages 201–240 of the volume I have so frequently cited already. The researches in this portion of my work, which are of a peculiar and novel character, lead to certain results which, according to my views, are indispensable for the progress of analytical physics. This remark will receive illustration from the use I now proceed to make of these results.

27. The mathematical consequences that are deduced from the above hypothesis relative to the composition of the differential $(d\psi)$, after taking into account the second and third general equations, are found to involve no contradictions, are definite and unique as regards both the motion and the density of the fluid, and account at once for certain observed facts which are due to essential properties of the fluid apart from arbitrary conditions, such as the fact of uniform propagation, and that of co-

existence of small vibrations. In the course of the reasoning it is shown (p. 202) that, corresponding to motion along a rectilinear axis, the function $f\phi$ has a *maximum* value, which is confirmatory of the antecedent argument in art. 25. As the investigation conducts to motions of a definite kind prior to any supposition as to the mode of disturbing the fluid, these motions may be called *spontaneous*, to distinguish them from those that result from arbitrary disturbances. The chief characteristic of these spontaneous motions is that they are *vibratory*, and that the directions of the vibrations are partly *parallel* and partly *transverse* to the axes of motion. Moreover the investigation shows, to whatever order of terms it be carried, that the vibratory motion, whether direct or transverse, is such that the movement of a particle in any direction is just equal to its movement in the opposite direction, so that there is no permanent motion of translation. This agrees with the principle stated in art. 10 of the Hydrodynamical Theory of Magnetism, according to which there can on the whole be no transfer of fluid across an unlimited plane having any position in a mass of fluid of unlimited extent. As in steady motions this necessary condition is satisfied by movements in complete circuits (see art. 10 above cited), so in unsteady motions it is satisfied by vibrations such as those above described; and hence it seems possible to perceive an *à priori* reason for the *spontaneity* of these vibrations.

28. In my researches on the Undulatory Theory of Light, I have shown that those phenomena which depend only on properties of the medium in which the light is generated, as especially the characteristics of a polarized ray, are readily explained by the laws of the spontaneous vibrations. Also by the coexistence and combination of such vibrations we may account for a beam of light of very small transverse section being transmitted to an unlimited distance without undergoing lateral dispersion, and for the limited lateral divergence of rays in phenomena of *diffraction*. But the phenomena which depend besides on the constitution of visible and tangible substances are referred by the theory to mutual action between the vibrations of the æther and the atoms of the substance. In this respect *light* is to be ranked with the physical forces, and its dynamical action is equally to be ascribed to pressure of the æther. In treating mathematically of this action, I have supposed the ætherial waves to be composite in such manner that the transverse vibrations are neutralized. This supposition requires that a^2, the exponent of the elastic force of the medium, should be changed into another constant a'^2, as is done, for a different reason, in the usual mode of treating hydrodynamical questions. Excepting what relates to this difference, the reasoning I employ in the dynamics of the

vibrations of an elastic fluid agrees, *to the first order of small quantities,* with that which is commonly adopted. For instance, I solve the problem of the resistance of the air to the vibrations of a ball-pendulum exactly as Poisson has done. Also the reverse problem of the acceleration of a ball by vibrations of the air, which is more to my present purpose, I have solved to the same approximation in an analogous manner. From the solution of the latter problem it appears that, so far as is indicated by the first approximation, ætherial waves produce vibratory motions of the atoms without permanent motion of translation. The impression of such vibrations on the atoms of nerves of the eye may be considered to be the proximate cause of the *sensation* of light.

29. In order, therefore, to determine whether the ætherial vibrations are capable of giving to atoms a permanent motion of translation, which is a question of essential importance in the proposed theory of the physical forces, it is necessary to proceed to the *second* approximation. The obvious way of doing this would seem to be to start from the first approximation, and then make use of it, according to the usual rules, in advancing to the second. But this course is liable to the objection that it does not differ, except in having a' in the place of a, from that which would be proper if it were unnecessary to take account of the effect of the spontaneous movements. Having, in fact, succeeded in overcoming the mathematical difficulty of effecting a second approximation by this means, I have ascertained that the solution contains terms of indefinite increase, whence it must be concluded that the logic of the process is somewhere at fault. The failure may, I think, be accounted for as follows. In the first approximation the effect of spontaneous motion is included by assuming that the actual vibrations result from an unlimited number of the spontaneous vibrations so combined as to neutralize the transverse vibrations; and this assumption is allowable for a first approximation on the principle of the coexistence of small motions. But this principle does not extend beyond the first approximation; and consequently, on proceeding to the second, additional steps are required for including the influence of the spontaneous movements.

30. Both in this Magazine and in my work on the Mathematical Principles of Physics, I have in various ways attempted to solve to the *second* approximation the problem of the motion of a small sphere acted upon by the vibrations of an elastic fluid. But I must confess that, owing to the difficulty of including the effect of the spontaneous vibrations, my efforts have been only partially successful. The arguments employed for this purpose in pp. 439–455 and in pp. 490–498 of the above-mentioned

work involve two unknown constants H_1 and H_2, for which no analytical expressions are obtained, and their values are consequently left undetermined.   On this account the theories which attribute the forces of *heat, molecular attraction,* and *gravity* to action on the atoms by pressure of the æther in vibration are incomplete.   Recently, however, a different course of reasoning has occurred to me, which, although it does not directly meet the difficulties of the second approximation, seems to give an intelligible account of the way in which vibrations of an elastic fluid are capable of producing an accelerated motion of translation of a small sphere.   This new process I shall now endeavour to explain.

31. Supposing at first the small sphere to be fixed, let a series of plane-undulations be incident upon it in a given direction. Then, if a line of abscissæ (x) be drawn in that direction through the centre of the sphere, it is evident that the whole of the motion and condensation will be symmetrical with respect to this line as an axis, and that the condensation (σ) on the surface of the sphere will be a function of x and t.   We may therefore make the general assumption that $\sigma = \sigma_1 f(x, t)$, and take σ_1 to be the condensation at the time t in the plane perpendicular to the axis through the centre of the sphere, so far as it is unaffected by the disturbance of the motion by the reaction of the sphere.   (I have reason from previous researches to say that throughout this plane, and therefore where it cuts the surface of the sphere, the condensation has that same value σ_1.)   The undulations are supposed to be incident in the positive direction of x.

32. Now there are *three* circumstances which determine the distribution of condensation on the surface of the sphere: first, the reaction of the sphere against the incident vibrations causes a certain amount of condensation, which is so much the smaller as the sphere is smaller, and for spheres the diameters of which are very small compared with λ is extremely small; secondly, after taking account of the condensation produced by this reaction, the proper condensation of the incident undulations is not *immediately* disturbed, and is therefore, *cæteris paribus,* the same at the surface of the sphere that it would have been at the same points if the sphere had not been there; but, thirdly, the distribution of the condensation is modified by the circumstance that the undulations are composed of spontaneous direct and transverse vibrations, in consequence of which, when they are disturbed by incidence on the sphere, *lateral* action is brought into play.   The kind of effect thence resulting may be conceived of as follows.   If undulations of very small breadth were incident on a very large sphere, the condensations which reach the further

half of the surface would be much diminished by reason of their limited lateral divergence on being transmitted beyond the first half. In extreme cases a portion of the fluid in contact with the second hemispherical surface might be altogether undisturbed. On the other hand, if waves of large breadth were incident on a small sphere, the condensations might become by lateral divergence greater on the second half surface than on the first, because the conditions of the motion would approximate to those of spontaneous motions along an axis, in which the condensation corresponding to a given velocity is greater than in plane- composite waves in the ratio of a'^2 to a^2. Accordingly, so far as the action of the spontaneous vibrations takes effect, the condensation on the surface might either *decrease* or *increase* as x is greater; but in the actual physical circumstances of the æther and the atoms the gradation must always be extremely small; otherwise, by reason of the vast elastic force of the æther, the acceleration of the atom would exceed the amount which experiment and observation appear to indicate.

33. This being understood, let θ be the angle which any radius makes with the part of the axis on the negative side of the sphere's centre, so that, if $x_{\prime}$ be the abscissa of the centre, $x = x_{\prime} - b \cos \theta$, b being the radius of the sphere. Also let $p = a'^2 \rho = a'^2 (1 + \sigma)$. Then we have with sufficient approximation

$$\sigma = \sigma_{\prime} f(x, t) = \sigma_{\prime} f(x_{\prime} - b \cos \theta, t) = \sigma_{\prime} f(x_{\prime}, t) - \sigma_{\prime} f'(x_{\prime}, t) b \cos \theta,$$

because, from what is said above, the value of $f'(x, t)$ must be very small and very nearly *constant*. Consequently the whole pressure on the sphere estimated in the positive direction is

$$\int 2 \pi a'^2 \sigma b \ \sin \theta \cos \theta \, d\theta, \text{ from } \theta = 0 \text{ to } \theta = \pi.$$

On substituting the above value of σ this integral will be found to be

$$-\frac{4 \pi b^3 a'^2 \sigma_{\prime}}{3} f'(x, t);$$

and consequently, if Δ be the ratio of the density of the sphere to that of the æther,

$$\text{the accelerative force} = -\frac{a'^2 \sigma_{\prime}}{\Delta} f'(x, t),$$

which is positive if $f'(x, t)$ is negative—that is, if the condensation *decreases* as x is greater.

34. With respect to the composition of the function f, so far as it depends on the *second* of the causes of distribution of condensation on the surface of the sphere mentioned in art. 32, it will contain only periodic terms, because the action of this cause is periodic. In fact it may be shown, just as in art. 19, that

the acceleration of the sphere by the proper condensation of the undulation has a constant ratio to the acceleration of the fluid where the sphere is situated. Now, since the fluid is just as much accelerated in the condensed as in the rarefied parts of the wave, the positive and negative velocities being exactly equal (art. 27), it follows that the sphere, not being of variable density like the fluid, is acted upon by *greater* accelerative forces when in the condensed part of a wave than when it is in the rarefied part. Since, however, for each part the *positive* accelerative forces are exactly equal to the *negative,* there is no residual accelerative action in either direction, and therefore no tendency to produce permanent translation.

35. I have ascertained by previous researches that the condensation due to the *reaction* of the sphere is, to the first approximation, so distributed that at each instant there is as much condensation on one half of the surface as rarefaction on the other half, and that they are similarly distributed about the axis. Also the variation of condensation at any given point of the surface follows the law of the incident undulations, so that the resulting accelerative action on the atom is *periodic.* When, however, it is considered that the reaction due to the incidence of the condensed portion of a wave must be greater than that due to the rarefied portion, quantities of the second order being taken into account, it will be seen that the accelerative action in the one case is not exactly compensated for by that in the other, and that there may be a residual action tending to produce permanent transfer. The effect will be extremely small on account of the very small condensations produced by the reaction, and may be considered to be taken into account by the process about to be applied to the distribution of density due to the *third* cause.

36. The law of distribution due to the third cause is independent of the time, being determined by the relative magnitudes of b and λ, the breadth of the undulations, or it may be by λ only. Hence, omitting periodic terms contained in $f'(x, t)$, which do not apply in the present research, and assuming, for reasons already given, that this function is equal to an unknown constant H, we obtain

$$\frac{d^2x_l}{dt^2} = -\frac{a'^2\sigma_l \mathrm{H}}{\Delta}.$$

At another epoch, and for a different value of the condensation, we might have

$$\frac{d^2x_l'}{dt^2} = -\frac{a'^2\sigma_l'\mathrm{H}}{\Delta};$$

for although the epoch be different, dt may be assumed to be

the same. Consequently

$$\frac{d^2(x_l + x_l')}{dt^2} = -\frac{a'^2 H}{\Delta}(\sigma_l + \sigma_l').$$

For taking the next step in the argument reference is to be made to a law already admitted (art. 27), according to which the vibrations of an unlimited elastic fluid must be such that for every acceleration and movement of a particle in a given direction there must be an equal acceleration and movement of the same in the opposite direction. Let therefore σ_l and σ_l' be condensations corresponding to movements so related. Then we shall have

$$-\frac{a'^2 d\sigma_l}{(1+\sigma_l)dx} = \frac{a'^2 d\sigma_l'}{(1+\sigma_l')dx}.$$

Consequently

$$\frac{d\sigma_l}{(1+\sigma_l)dx} + \frac{d\sigma_l'}{(1+\sigma_l')dx} = 0,$$

or

$$d \cdot (\sigma_l + \sigma_l') + d \cdot \sigma_l \sigma_l' = 0.$$

Hence, by integration,

$$\sigma_l + \sigma_l' + \sigma_l \sigma_l' = 0,$$

no arbitrary constant being added, because plainly σ_l and σ_l' must vanish together. This result shows that σ_l and σ_l' must be one positive and the other negative. Let σ_l' be negative, so that it belongs to the *rarefied* portion of a wave. Then, since $\sigma_l' = -\dfrac{\sigma_l}{1+\sigma_l}$, apart from sign σ_l' is less than σ_l which belongs to the condensed portion; as plainly should be the case. Also, from the above equation,

$$\sigma_l + \sigma_l' = -\sigma_l \sigma_l' = \frac{\sigma_l^2}{1+\sigma_l} = \sigma_l^2 \text{ nearly.}$$

Therefore

$$\frac{d^2 \cdot (x_l + x_l')}{dt^2} = -\frac{a'^2 H}{\Delta} \sigma_l^2.$$

This equation gives the *residual* acceleration of the atom when the actions upon it at any two corresponding positions in the condensed and rarefied portions of an undulation are taken into account simultaneously; whence it may be inferred that the total condensations and rarefactions accompanying every complete vibration of the fluid backwards and forwards produce on the whole an acceleration of the sphere in the direction of the propagation of the waves if H be negative, and in the opposite direction if that quantity be positive. These effects correspond respectively to *repulsive and attractive forces*.

The value of σ_i at any time t at a given position being expressed by the function $\mu \sin\left(\dfrac{2\pi a't}{\lambda} + c\right)$, we shall have

$$\sigma_i{}^2 = \frac{\mu^2}{2}\left(1 - \cos\left(\frac{4\pi a't}{\lambda} + 2c\right)\right),$$

showing that together with a periodic part the force $-\dfrac{a'^2 H}{\Delta}\sigma_i{}^2$ has a constant part, which is proper for giving to the sphere a constantly accelerated motion.

37. If the sphere, instead of being fixed, were free to move in obedience to the accelerative force, the acceleration of its motion would give rise to resistance from the inertia of the medium; but it might be shown, just as in the case of acceleration of a sphere by fluid in steady motion, that the only effect would be to diminish the acceleration in a certain constant ratio.

38. If the series of incident waves be propagated from a centre, μ, as is known, varies inversely as the distance from the centre, and therefore μ^2 and the accelerative force would vary *inversely as the square of the distance*, in agreement with the *law of gravity*. Since it is known by experience that gravity accelerates all atoms equally, in applying the foregoing formula to account for the laws of gravity, it must be supposed either that all atoms are of the same size, or that, for vibrations proper for producing the observed effects of gravitation, the factor H is independent of the magnitudes of the atoms. The latter supposition is, I think, more likely to be true than the other.

39. If the value of σ_i be expressed by the sum of any number of terms such as $\mu \sin\left(\dfrac{2\pi a't}{\lambda} + c\right)$, we should have

$$\sigma_i{}^2 = \Sigma\left[\frac{\mu^2}{2}\right] + \text{ periodic terms };$$

from which it follows that the whole acceleration is the sum of the accelerations due to the separate terms. In the proofs of Propositions XV. to XVII. contained in pp. 225–239 of the 'Principles of Mathematics,' I have demonstrated that those condensations in different sets of vibrations which correspond to non-periodic terms of the second order may *coexist*; so that the results obtained by extending the investigation to cases of the propagation of different sets in different directions in space account generally for the coexistence of all attractive and repulsive forces which are referable to *vibrations* of the æther.

40. The phenomena of light indicate the existence of an unlimited number of coexistent ætherial vibrations for which λ has every gradation of value within certain limits, and make it pro-

bable that beyond these limits others exist not recognizable by the power of vision. According to the hydrodynamical views I maintain, the special form of the simple vibration and the values of λ result spontaneously from a disturbance without being determined by the particular character of the disturbance (see arts. 13–15 in the Theory of Magnetism, June Number). Of all orders of vibrations those may be presumed to have the least values of λ which emanate from individual atoms, and are due to the aggregate of reactions at their surfaces consequent upon the incidence of vibrations from all surrounding quarters. According to a foregoing argument (art. 32), it may be assumed that, for undulations of this order incident on an atom, H will be a negative quantity, and consequently that any given atom is always repulsive towards surrounding atoms. This is the theory of the force I call *atomic repulsion*, which I consider to be the same as the repulsion of *heat*.

41. The vibrations emanating from a vast number of atoms constituting a *molecule* may be conceived to produce by their composition vibrations of another order, in which the characters of the component vibrations are obliterated and the values of λ are much larger. If we may suppose that for vibrations of this order H has a *positive* value, we shall be able to account for a *molecular attraction* acting so as to control at the boundaries of substances the atomic repulsion, and thus have the effect of maintaining the atoms, both superficial and interior, in positions of equilibrium. Assuming that H may have both a negative and a positive value, we might infer that for certain values of λ that quantity either vanishes or is very small. Possibly these may be the values for light-undulations, which seem to possess vibratory rather than translatory power. The whole theory, however, of attractive and repulsive forces, regarded as due to vibrations of the æther, is incomplete for want of an *à priori* determination of the composition and value of the quantity H.

42. The composite class of vibrations to which the theory ascribes molecular attraction must, by the mere effect of separation caused by propagation to a great distance, be resolved into the component primitive vibrations, which will again act repulsively, and require to be controlled by another order of attraction-vibrations, and so on till we come to the attraction-vibrations of gravity. Moreover, as it is impossible to account for the stability of the general system of stars, or of systems of stars which constitute resolved nebulæ, if the individuals simply attract each other by the force of gravity, it seems reasonable to suppose that the gravity-vibrations may in like manner be resolved by propagation to vast distances into repulsive components. In this manner neighbouring stars would be mutually

repellent, while the whole stellar system might be kept together by a still higher order of attraction-vibrations resulting from composition of the vibrations from all the individual stars. This theory would receive confirmation if observation should eventually decide that the proper motions of stars are *vibratory*.

I have now adverted to the more essential parts of the theories of all the physical forces, and have endeavoured to account for their modes of action by mathematical arguments founded on the hypothesis that all the forces and their laws are ascribable to pressures of the æther. The extent and comparative facility of the applications of the deductions from this hypothesis in accounting for observed facts and laws seem to justify the conclusion that it is (to adopt an expression employed by Whewell in his ' Inductive Sciences ') "the appropriate idea" of Physics.

Cambridge, July 24, 1872.

XXV. *On Wave-Theories of Light, Heat, and Electricity.* By Henry Hudson, *M.D., M.R.I.A.*[*]

THE withering influence which the authority of a great name exercises over the views of men of science is perhaps nowhere so strongly marked as in the theory of optics. If the genius of Fresnel had not arisen to develope the views of our own Young (to whom we owe not only the idea of "transversal vibrations" but also the grand conception of the "interference of light"), the emission-theory of the illustrious Newton might have continued much longer to check the progress of true science.

Although the undulatory theory may be said now to be completely triumphant, it must still be admitted that there remain certain phenomena which require explanation, and I have long thought that there is *one* very weak point in the physical conceptions upon which the wave-theory has been founded.

Huyghens (in order to explain the phenomena of double refraction) assumed the existence of a *second vibrating medium,* consisting of the æther and molecules of matter *conjointly*; and Young as well as other eminent philosophers have adopted this view. This assumption is, I believe, erroneous. We know that waves of sound in our atmosphere are 10,000 times as long as the waves of light, and their velocity of propagation about 850,000 times less; and even when air has been raised to a temperature at which waves of red light are propagated from matter, the velocity of sound-waves is only increased to about

double what it was at zero Centigrade. Even their velocity through glass is 55,000 times less than the speed of ætherial undulations, and the extreme slowness of change of temperature in the "conduction of heat" (as contrasted with the rapidity with which the vibrations of the æther exhaust themselves, becoming insensible almost instantaneously when the action of the exciting cause ceases) marks distinctly the essential difference between molecular and ætherial vibrations. It appears to me, therefore, a very crude hypothesis to imagine a combination of ætherio-molecular vibrations as accounting for the very minute difference in the retardation of doubly refracted rays in crystals.

Among the known facts which remain unexplained by Fresnel's theory the following are prominent:—

1st. That no interference can take place between two rays originally polarized in perpendicular planes, even when they have been brought into the same plane of polarization.

2nd. The rings from thin plates (observed by Arago) when viewed through Iceland spar with its principal section parallel or perpendicular to the plane of incidence—the result being that the light of *both* the transmitted and the reflected rings is wholly polarized in the *plane of incidence,* the colours being complementary and their intensities perfectly equal.

3rd. That, in common light, in order to explain the phenomena of interference, it is necessary to admit a sudden transition (*per saltum*) from one *system* of waves to another, in which the vibrations are altogether different and have no apparent connexion.

4th. The case of double refraction in crystals of quartz appears to afford another example of the inadequacy of the theory of a system of waves propagated through a single elastic medium, inasmuch as it presents a total *breach of continuity* in the transition from the velocity of the one ray to that of the other.

5th. The phenomena of "absorption," which will, I believe, be ultimately explained through the principle of interference.

We know, from the aberration of the light of the fixed stars, that "the æther" encompassing our earth does not participate in its motion; and therefore it is only the "excess" of æther, associated with the molecules or atoms of matter, which is carried along with it in its motion through space; and we can have no doubt with respect to the sameness of the density and elasticity of the æther throughout space, inasmuch as the velocity of the propagation of light (which depends on them) is the same whatever be its origin.

How, then, can we explain such theoretical difficulties as I have alluded to? I believe it will be necessary to consider what we term the æther as consisting of *two media,* each possessed of

equal and enormous self-repulsion or elasticity and both existing in equal quantities throughout space, whose vibrations take place in perpendicular planes, the two media being mutually indifferent, neither attracting nor repelling.

Common light, therefore, will consist of waves (equal in every respect and undistinguishable by our organs of vision) in *each* medium, the vibrations being in a plane perpendicular to the direction of the wave's progress.   But the reflected ray in the one medium will, I conceive, present the qualities of the refracted ray in the other; and it is manifestly impossible, no matter how perfectly similar they may appear to be, that any interference can take place between waves thus propagated independently in different media.

By this slight modification of the physical conception on which the wave-theory is founded I believe every difficulty will be removed, and the cause of the equal quantities of polarized light obtained by reflection and refraction is obvious.

I think we have a proof of the existence of two distinct media in the æther in the fact that the "ordinary" and "extraordinary" rays (when produced by two similar plates of the same crystal in *rectangular positions*) *will interfere*, whereas the two rays of the *same denomination* (even when brought into the same plane) are incapable of interference.   The reason is obvious if the two ordinary rays (thus produced) are in different media, while the ordinary ray from one plate and the extraordinary ray from the other are in the same medium.   As a further test of the hypothesis, I beg to suggest an attempt, experimentally, to make the "ordinary refracted" ray (through Iceland spar) show any indications of interference with the apparently similarly polarized ray obtained by total reflection.   As I have already said, I believe they are produced by vibrations in different media, and must uniformly increase the illumination (instead of producing darkness or fringes), no matter how either ray may be retarded in its path.

With regard to calorific waves, I believe that they are due to those vibrations of the æther which take place in the *direction of the wave's progress*, and, as such, are totally distinct from luminiferous undulations.

In reference to luminiferous (or transversal) vibrations, Fresnel has shown that, " in such a medium as the æther, the force which resists the approach of two strata is *much greater* than that which opposes their sliding on one another."   Poisson in his researches (1830) points out that, according to his theory, there will be *two* waves, the vibrations in one being " in the direction of propagation and attended with dilatations proportional to the absolute velocity of the molecules " (this I therefore look upon

as the "calorific wave"), "the vibrations of the second wave being transversal and unaccompanied by any change of density in the medium." The latter is therefore my "luminiferous wave."

Again, according to Cauchy's investigations, "a ray entering any medium will be subdivided into *three* rays, and, when the elasticity of the æther in this medium is the same in all directions, the three will have a common direction and two of them a common velocity, being thus reduced to two, viz. a single ray and a double ray—the vibrations of the former [my calorific ray] being in the direction of propagation of the wave, and those of the double ray transversal," constituting (on my view) common or unpolarized light.

It may be observed also that Fresnel's theory admits that the vibrations of polarized light *may be* either parallel or perpendicular to the plane of polarization ; but, finding that the vibrations of the "ordinary ray" (by double refraction) are perpendicular to the principal plane of the crystal, which is the plane of its polarization also, he drew the conclusion that the vibrations of *all* polarized light *must be perpendicular* to the plane of polarization. On the other hand, Cauchy's researches led him to the conclusion that the vibrations of a polarized ray are *in the plane* of its polarization. This apparent difference in the results of these two eminent analysts may, I should hope, be reconciled (on my view) by supposing that Cauchy's investigations refer to the "ordinary ray" polarized by reflection ; for its vibrations I consider to be in the plane of its polarization. If I am right, therefore, I think we shall have obtained a most satisfactory general accordance between the able researches of the three great French mathematicians.

Although the disturbance of the æther associated with the molecules of a body, by a luminiferous wave, is very trifling, I believe nevertheless that it is to this cause we must look for the explanation of the phenomena of phosphorescence and also of fluorescence. Molecular vibrations being, as we know, comparatively very sluggish, are required to maintain the *continuance* of the ætherial (phosphorescent) vibrations, which must otherwise be expected to subside almost instantaneously (on the withdrawal of the exciting cause) if not sustained by the reaction of the associated molecules, in like manner as heat-waves (in the conduction of temperature) are maintained after the source of the heat has been withdrawn.

With regard to the nature of calorific waves, long before I had arrived at any definite views I felt compelled to reject the idea of their consisting in *molecular* motion, for the following reasons :—1st. Because they are propagated through space, where

every indication is opposed to the existence of matter in the ordinary sense. 2nd. To suppose an ætherial wave impinging on a body to act *primarily* on its material molecules (instead of upon the associated æther) appears to me a most gratuitous and unlikely assumption. 3rd. In the case of *free transmission* of light or heat-waves through any substance without producing (so far as we can perceive) the slightest disturbance of the molecules of the body (evidenced by phosphorescence or increase of temperature), I find it impossible to admit Tyndall's conclusion, viz. "that free transmission indicates discord and absorption concord." If the vibrations of an impinging ætherial wave *interfere* with those of the æther (associated in the molecules), there must be disturbance of the latter, which will, no doubt, cause disturbance of the molecules also, and hence, on my view, "phosphorescence or increase of temperature." 4th. The existence of very intense light without any appreciable heat (as in moonlight concentrated by lenses or mirrors), as also of great heat from perfectly dark matter, compelled me to discard the idea that light and heat could *only* differ in refrangibility or wave-length. The fact also that a compound beam of light and heat can be so easily deprived of either of its constituents by transmission through different substances, as well as the different march of the luminiferous and calorific rays (of the same refrangibility) in respect to their intensities, all pointed to the same conclusion, viz. "that both sets of waves could not be ascribed to the *same function* of the ætherial vibrations;" and from this consideration arose the idea that calorific waves are probably due to ætherial vibrations in the direction of the wave's progress.

A theory, however, may be plausible (as I hope this will appear) and yet require proof, which the theory itself, if true, ought to supply. I have already indicated an experimental test in reference to two *media*, and beg now to suggest that Foucault's experiment of the revolving mirror (with lenses of rock-salt and a thermopile) may be applied to ascertain the *velocity* of calorific waves. The resistance to wave-progress where the vibrations are normal must be considerably greater than in the case of transversal vibrations; and, as a consequence, the progress of the calorific wave ought to be considerably slower than that of the luminiferous wave.

I may add that the "conduction of heat" in crystals is, so far as I am aware, better in the direction of their *planes of cleavage* (as in bismuth) than in any other direction, and, with wood, in the direction of the fibre, facts which appear to me in favour of my views, that calorific vibrations are in the direction of wave-progress; the molecules gliding more easily and attaining greater

amplitude of vibration in the interstices of the planes of cleavage, in which direction we must necessarily conclude that the ætherial waves (which act on the molecules) must also have their vibrations.

I have classed fluorescence with phosphorescence:—1st, because none of the fluorescent rays are transmitted; and 2nd, because they are *not* polarized even when the light which produced them *was* polarized. Now I find it difficult to admit the mere *change of refrangibility* of the incident rays; but to believe them to be also *depolarized* is, in my mind, impossible.

So far as I am aware, the alleged " polarization of heat " is the only serious objection to my views; if calorific waves be *normal* (not transverse) there *can be* no polarization. Melloni has shown that Forbes's experimental proofs cannot be accepted; and I have never been satisfied with Melloni's own experiments. The velocity of heat-waves is, I believe, the point which must decide the question; but I conceive it to be possible that the consideration of those waves whose vibrations are *intermediate* between the direction of progress and its perpendicular may account for some small indications of heat polarization, as such waves undoubtedly would partake in some degree of the qualities of both luminiferous and calorific undulations.

Having thus discarded molecular vibrations as causing " calorific waves," in order to satisfy the experimental researches of Rumford, Davy, Joule, Tyndall, and other philosophers we have only to substitute " temperature " in the place of heat; for, to my thinking, temperature is simply an *accident* of matter, and represents the state of motion of its molecules under the influence of their associated æther. I look, therefore, on " temperature of space " as a pure myth; and I believe a zero of temperature to be an utter impossibility; for, in a world constituted like our universe, absolute rest is unattainable, and equally absurd with " perpetual motion."

I believe that the dark lines in the solar spectrum are due to the interference of waves of like refrangibility, which differ in their periods by an odd number of semiundulations, and consider the same explanation applicable to certain phenomena of heat which have generally been referred to the theory of exchanges.

According as the quality of the radiation (as regards velocity of vibration) approaches more nearly to that of the æther associated with the molecules of matter through which we transmit the rays, the greater will be the tendency, I conceive, to an increase of interference. Thus, for instance, the most transparent glass loses almost entirely its power of transmitting light when its temperature is so raised that it becomes " self-luminous;" and even below a red heat its transparency is greatly diminished.

In this case it is obvious we cannot have recourse to a "theory of exchanges;" and yet the explanation of the phenomena must, I conceive, on the undulatory theory be similar for both light and heat. I may add that a diminution of the index of refraction produced by increasing the temperature of a medium (as shown experimentally by Gladstone, Dale, and Landolt) appears an obvious consequence of the views here indicated.

Whether we consider the case of sound, light, or heat, there can be no doubt that "intensity" depends on the amplitude of vibration, and quality (viz. pitch in music, and colour or refrangibility in light and heat) depends on the number of vibrations in a given time. Increased temperature thus represents greater amplitude of the vibrations of the molecules of matter as contradistinguished from those of their associated æther, which, however, we must consider as generally *synchronizing* with the former, although their number per second must be enormously greater.

When we consider molecular motions separately, we can scarcely doubt that the momenta of the molecules (V × W) of two bodies at the same temperature are equal; that is to say, the "velocity of vibration" (in chemical language "the capacity for heat") varies *inversely* as the molecular weight—a deduction which is in accordance with experiment.

A curious consequence appears to flow from these views when we compare the same substance at the same temperature in *different states of aggregation.* Thus water and ice at zero Centigrade have the same molecular momentum. But the specific heat of ice is only one half that of water (viz. as 0·5050 : 1), therefore the molecule of ice should be to that of water as 2 : 1 (to maintain their equal momenta). Again, in comparing water and steam (at the boiling-point), their specific heats (as 1 : 0·4805 per Regnault) *appear* in like manner to indicate a ratio of 2 : 1, whence it would in fact follow that the molecular weight of water is only one half of the molecular weight of either ice or steam ! Thus if the molecular weight of steam be taken as 9 (in accordance with hydrogen's *gaseous unit*), then the molecule of water can only be 4·5, and the *atomic* weights of hydrogen and of oxygen cannot exceed 0·25 and 4 respectively; so that the *gaseous* molecule of both hydrogen and oxygen must consist of at least 4 atoms each !

With regard to electrical phenomena, it appears to be abundantly proved by Faraday that the two electricities exist in equal quantities associated with the atoms or molecules of matter, and the amount is so great that the electricity evolved from 1½ grain of water (decomposed) is adequate to charge 500 acres of cloud surface; they are *each* self-repellent, and are both attracted by

matter, from which they can be separated and a portion transferred at pleasure from one surface to another without losing their distinctive properties. I believe, therefore, that they must be admitted to have a *real existence,* and cannot be regarded as mere motion or as properties of matter. Assuming this, it appears to me that Wheatstone's experiment with the revolving mirror leaves no doubt that electricity consists of *two* self-repellent fluids, and can no longer be considered merely the excess or deficiency of a single fluid.

In addition to the above properties, it is generally assumed that there is a mutual attraction between the two fluids; but a careful consideration of the phenomena has led me to the conclusion that this opinion is erroneous, and that they are *mutually indifferent.*

An experiment of Mohr's appears to me strongly in favour of this view; viz. when an electrized body was placed at 1 centim. from one end of an insulated cylinder (65 centims. long), the *neutral point* was found to be at only 1 centim. from this extremity of the cylinder. If attraction existed between the two electricities, we should expect a large accumulation of the opposite fluid at the end of the cylinder nearest to the charged body, while the actual result appears plainly to indicate MERELY a repulsion of the electricity of the same denomination with that of the charged body.

The facility with which the two fluids are separated even by the slightest friction, and their not instantly disappearing again (as separate entities) when eliminated, first excited a suspicion on this point, which has grown up into a firm belief that all the statical phenomena supposed to establish mutual attraction can be accounted for by the attraction of each for matter and the *self-repulsion* of each fluid. Assuming, therefore, this to be the constitution of the two electricities, and feeling bound to admit no more causes than are necessary to account for phenomena, I have arrived at the conclusion that "the two electricities," in equal quantities, each possessing self-repulsion (and mutually indifferent to each other), possess those qualities which are, in my mind, essential to a thorough explanation of the undulatory theory, and constitute by their diffusion throughout space that " æther" whose existence is generally admitted as proved by the phenomena of optics—the luminiferous vibrations of the two fluids taking place in perpendicular planes, and always transverse to the direction in which the light is propagated.

On this hypothesis the cause of the intimate connexion between the state of the sun's photosphere and magnetic storms, as well as other terrestrial electromagnetic phenomena, becomes obvious; and, as regards affections of molecules, I recognize in

their associated æther the principle of elasticity, or of that re-pulsive force which equilibrates the attractions of molecules or atoms *inter se*.

With regard to Boscovich's theory of *alternations* of attraction and repulsion between particles themselves at minute distances, I reject it as irreconcilable with the phenomena, inasmuch as at *any* temperature a body resists either compression or extension; so that its actual state must result from an equilibrium of two opposing forces, viz., on the one hand, the mutual attraction of the molecules (which even with gases must be allowed to exist at whatever distance, inasmuch as gravity has no known limit, and the attraction which our earth exercises over gases at their extreme rarefaction is an undoubted fact), and, on the other hand, the self-repulsion of the æther associated with the molecules, which may thus (in chemical philosophy) take the place of the "caloric" of former times.

In accordance with these views, electricity should be looked on in future (in chemical theories) as causing *decomposition* by se-parating combined molecules, and thus allowing the action of various forces of attraction to play their allotted parts in the changes which take place.

The views of the illustrious Faraday on this subject appear to have varied much from time to time: although he admits that chemical combination alone never eliminated electricity, and that "decomposition" was an essential in voltaic arrangements, still, on the whole, his leaning appears to have been to the views of Davy, "that electricity was the cause (not the opponent) of che-mical affinity" (Researches, 858–861).

With respect to electrical phenomena, it appears to me that there are "waves of translation" as well as waves of vibration, the former being always productive of the latter, and the prin-cipal agents, as such, in dynamic electricity. The "parallelism" of the quality of conductivity in bodies (for temperature and for electricity) corroborates the view I have taken of the intimate connexion of the motions of molecules with those of their asso-ciated æther—the former sluggish and persistent, the latter amazingly rapid and evanescent, while both present the same *relative* velocities in passing through the same substance*.

In conclusion I would submit that many erroneous theories appear to have arisen from regarding the ultimate particles of a body as *spheres*. They might be spheroids or ellipsoids; or more probably they may have the *simplest* forms of those crystals which they build up by their mutual aggregation; but taking into account the *polar* forces which come into play at insensible

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* Dessaignes first observed that "*bad conductors of electricity* are easily rendered *phosphorescent*, good conductors rarely, if ever."

distances, the sphere appears to me the most improbable of all forms.

The law of attraction (as $\frac{1}{D^2}$) I conceive to exist at *all* distances (subject to a polar force which becomes insensible at minute distances); and with regard to chemical equivalents or atomic weights, whether they are due to an unchangeable difference of MASS (in the sense of gravitation) of the ultimate particles, or are essentially *different forces* of what chemists have termed "affinity," we must not forget that they in truth only represent "the mean force of attraction exerted between OUR EARTH (as a whole) and the combining quantities of those different substances."

XXVI. *On the Law of Gaseous Pressure. By* the Hon. J. W. STRUTT, *M.A., late Fellow of Trinity College, Cambridge.*

IN reply to Mr. Moon, I will consider first the objections which he offers to the received theory.   In the Philosophical Magazine for July 1868, four particular cases of the problem in one dimension are considered, in each of which the law $p \propto \rho$ is supposed to lead to error.   The first is reprinted in the August Number.

A cylinder contains air which is at rest, but whose density varies discontinuously in crossing a certain section.   Such difficulty as the problem presents appears to me to be purely of a mathematical character, arising out of the discontinuity.   In any case of fluid motion the possibility of a difference of pressure at two points, P and Q, finitely distant, depends on the *inertia* of the intervening air and its consequent resistance to acceleration.   If, as in ordinary cases, the acceleration be finite, the difference of pressure will tend to zero as P and Q approach one another, without limit, because this is true of the inertia.   But the conclusion no longer follows if there be an infinite acceleration.   The reaction of an infinitely small mass to an infinite acceleration may be as finite as that of a finite mass to a finite acceleration.   Now the layer of air situated at the boundary *is* subject to an infinite acceleration, and therefore, no matter how thin it may be taken, its resistance to acceleration cannot be left out of account.   That the pressures which act on its two faces are unequal is, therefore, not in contradiction to any true principle.

Mr. Moon's other arguments depend also, as I believe, on logical fallacies in the treatment of infinitesimals.   The second supposes the case of "a vertical cylinder closed at its lower end,

and having an air-tight piston capable of moving freely in the upper part of it.   Below the piston the cylinder is filled with air, which is kept in equilibrium by means of a weight, W, resting on the piston, above which there is a vacuum."   If a second weight, W, be placed on the piston, we know that the equilibrium will be destroyed ; but, according to Mr. Moon, if the received law of pressure were true, such ought not to be the case.   Even Mr. Moon must admit that it is remarkable that so apparently reasonable a law should lead to such an absurd conclusion.   Of course, it is easy enough to deduce the opposite result from the same premises.   If the piston do not descend, it must be supported by a pressure which is confessedly inadequate.

So far as I understand it, Mr. Moon's argument may be expressed thus :—The lamina of air beneath the piston will not begin to move until the pressure exercised on it by the piston (equal to its own pressure on the piston) has changed.   And the pressure of the lamina (by hypothesis) cannot change until there has been a relative displacement of its parts, which requires that the motion should have already begun.   It would appear, therefore, that the downward motion of the lamina (and piston) cannot begin.   Precisely the same argument may be used to prove that a body cannot begin to fall under the influence of gravity ; for a body cannot leave its initial position without acquiring velocity, and (by the law of energy) cannot possess a velocity without having already fallen.   An argument of this kind is destitute of validity ; and its conclusion may or may not be true. In the case of gravity, where $v^2 \propto s$, we all know that falling from rest is possible ; but if the law of motion be that v simply varies as s, it is true, as may easily be shown, that a body once at rest cannot begin to move.   In order to arrive at a safe conclusion by the method followed by Mr. Moon, a much closer consideration of the order of the infinitesimals concerned is indispensable.   A nearly similar objection would apply to Mr. Moon's treatment of cases 3 and 4*.

A remark of a more general character may be made, which in most people's judgment would be sufficient to dispose of the question.   Mr. Moon proves a little too much ; his arguments, if valid at all, would establish that the received view is not merely false, but self-contradictory.   Thus in case 2, starting from identically the same premises, we prove by two different lines of rea-

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* Mr. Moon assumes, if I rightly understand him, that if a state of things once exist, and it can be shown that, *whenever* it does exist, the rate of departure from it vanishes, then the state is necessarily permanent. On this principle it would follow that a curve once meeting the axis of x, and never meeting without also touching it, necessarily coincides wholly with it.

soning (1) that the weight W will fall, and (2) that it will not fall. The same may be said of the other arguments. Now, whether air obey Boyle's law or not, surely an ideal fluid may be imagined which shall do so without any inherent contradiction. However interesting Mr. Moon's problems may be as logical paradoxes, they convey no information on the physical question which is under discussion.

In his restatement of the analytical arguments by which it is proved that p is a function of v, Mr. Moon has made some alterations the effect of which is to obviate the objection that I urged in the July Number.

" If we have three relations,

$$p=f_1(xt), \quad \rho=f_2(xt), \quad v=f_3(xt),$$

where the forms of f_1, f_2, f_3 are utterly unknown to us, the presumption is that $p=$ funct. (ρ, v). This is the rule, to which there may possibly be exceptions; but those who rely on the exceptions must prove them to exist."

On this I have two remarks to make. In the first place, I had understood (as it now appears mistakenly) that the argument was put forward to prove that p was necessarily a function of v. Understanding *may* instead of *must*, the reasoning is beyond cavil. But the objection is only transferred to the premises; for, of course, I entirely deny that the forms of f_1, f_2, f_3 are utterly unknown to us. On the contrary, I assert that $f_1 \propto f_2$; and even Mr. Moon admits that we have this *à priori* knowledge of the forms of the functions in the case of relative equilibrium. I quite agree with Mr. Moon that the attempt to extract Boyle's law from the sole conditions that p, ρ, v are functions of x and t subject to

$$\frac{dv}{dt}=\frac{1}{D}\frac{dp}{dx}, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (A)*$$

must necessarily fail; but this is only because some of the requisite conditions have been omitted. The reason why I reject his expressions for the pressure, velocity, and density is simply that, though they satisfy the conditions he prescribes, they do not satisfy the conditions that I prescribe.

If Mr. Moon has really obtained the most general values consistent with (A) (a point on which I am scarcely competent to judge), they necessarily include all the results of the received theory. Only so far would they have any application to gaseous dynamics, though from a purely analytical point of view their value may be very great.

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* Another equation should properly be added,

$$\frac{d}{dt}\left(\frac{D}{\rho}\right)=\frac{dv}{dx}.$$

In the second place, I wish to observe that even if Mr. Moon were right in rejecting Boyle's law when there is relative motion, it would still be without physical significance to express p as a function of v. That this might be done in any *particular case* of motion in one dimension is admitted; but the statement, though true, would express, as it stands, no physical law. What the pressure at any point of a fluid in motion may be under prescribed circumstances is (as I said before) a purely physical question; and we are agreed, I believe, that the magnitude of the actual velocity at the point, whether measured absolutely or relatively to other parts of the fluid at a distance, has nothing to do with it. Under these circumstances the expression of p as a function of v in a particular case, though it may be correct, is not instructive. Perhaps an illustration will render my meaning clearer. In the actual course of events the length of Mr. Moon's hair and the length of mine are both functions of the time. Eliminate the time and the length of our hairs are expressed as functions one of another; but no physical connexion is thereby proved. Again, in the motion of the earth round the sun, the force of attraction exercised upon it is (according to received views) proportional to r^{-2}; but inasmuch as r is a function of θ (the angular coordinate), or of the time t, the force of gravity may without error be expressed in terms of θ and t—either separately, or jointly in an indefinite number of ways. Yet no one, I imagine, would say that the law of gravity could properly be so stated. It is therefore desirable that Mr. Moon should state what he conceives to be the real physical law of pressure, true at all times and places.

It is perhaps abstractedly possible that besides ρ the differentials of ρ and v with respect to space may be elements on which the pressure depends. (That the velocity itself is not an element in the matter is, I believe, admitted by Mr. Moon, and is at any rate a direct result of experiment.) Indeed any theory of gases which professes to be more than a first approximation, must give an account of the tangential force which acts between two portions of the gas separated by an ideal plane, whenever the first differentials taken normally of the tangential velocities of the gas in the neighbourhood of the plane are finite. Of course nothing of this kind can happen when the motion is in one dimension merely.

I do not know what Mr. Moon may think of the kinetic theory of gases; but it has certainly great and increasing claims to be considered at least a truthful representation of the facts. The dependence of pressure on the density (and temperature) only, is one of the simplest consequences of its fundamental assumptions.

In the absence of any other even plausible theory, and in view of the fact that all its *legitimate* consequences are in perfect harmony with observation whenever they can be brought to the test, the received law of pressure is, I maintain, the only reasonable one.

XXVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 145.]

May 16, 1872.—Francis Galton, M.A., Vice-President, in the Chair.

THE following communication was read:—

"On Supersaturated Saline Solutions.—Part III. On a relation between the Surface-tension of Liquids and the Supersaturation of Saline Solutions." By Charles Tomlinson, F.R.S., and G. van der Mensbrugghe.

It was stated by one of us in Part II.* that when a drop of a liquid is deposited on the surface of a supersaturated saline solution, it will do one of three things—(1) mingle with the solution without any nuclear action, (2) spread out into a film with powerful nuclear action, or (3) assume the form of a lens, without any separation of salt. It was further stated that when a liquid forms a film or a lens, it does so according to the general proposition, that if a drop of a liquid B, with the surface-tension b, be placed on the surface of another liquid A, with the surface-tension a, the drop will spread into a film, if $a > b + c$ (c being the tension of the common surface of the liquids A and B); but if, on the contrary, $a < b + c$, the drop will remain in the form of a lens. Hence, if B spread on A, A will not spread on the surface of B. When the liquids A and B mingle in all proportions, c has no value. The spreading of the drop may also be interfered with by the superficial viscosity of the solution, or the greater or less difficulty in displacing the superficial molecules.

It was also stated that if a greasy smear be made upon the clean interior surface of a flask above the solution, and the flask be inclined so as to bring a portion of the solution against such smear, the liquid does one of two things: (1) it breaks up into well-defined globules, which roll over the smear without loss of tension, in which case the smear has no nuclear action; or (2) as soon as the solution reaches the smear its edge flattens and becomes ragged, in which case the smear is nuclear and the salt separates.

A glass rod drawn through the hand becomes covered with a smear or film; or the same rod, by exposure to the air, contracts a film by the condensation of floating vapour, or a deposit of film-forming dust, and so is brought into the nuclear condition.

It was further stated that when a lens of oil is resting on the surface of a solution, the flask may be rapidly rotated or briskly shaken,

* Philosophical Transactions for 1871, p. 52.

so as to break up the oil-lens into a multitude of minute globules, giving the solution the appearance of an emulsion—and that by repose the solution regains much of its transparency, without any separation of salt—but that if, while the flask is being turned round, a sudden jerk be given to it, so as to flatten some of the globules against the side, the solution instantly becomes solid.

The powerful action of films in putting an end to the state of supersaturation being thus established, it occurred to one of us, who had already succeeded in explaining a number of obscure phenomena on the principle of surface-tension*, that that force, properly handled, would suffice to account for most, if not all, the varied phenomena of supersaturation. According to this view, whatever tends greatly to lower the surface-tension of a supersaturated saline solution, causes a separation of salt, and at once puts an end to the condition of supersaturation.

In order to test this view, a large number of experiments have been performed by one of us during the last six months, consisting of repetitions of former experiments or of new ones suggested by one or both of us. All these experiments have been performed in the open air at Highgate, near London, the object being to avoid all possible miscarriage from the effects of floating dust in the air of a room. It had been suggested that some of the former results as to the action of films might have been vitiated from this source ; and although this does not appear to have been the case, yet it is with much satisfaction that the experimenter refers to the greater facility and certainty with which experiments of this kind are conducted in the open air, as compared with those made in a room. In the open air a gentle wind would sometimes blow over the mouths of the flasks, sufficient to produce a low musical note, without any nuclear action, unless a speck of soot or a small insect were carried into the solution ; but in general, in order to prevent evaporation, the flasks were kept covered with watch glasses or small beakers, except when performing an experiment.

The salt used in the following experiments was sulphate of soda, in large crystals, not effloresced, one of three strengths being adopted as circumstances required, which will be indicated when necessary —namely, 1 part of salt to 1 of water, 2 parts of salt to 1 of water, and 3 parts of salt to 1 of water. Every solution was first made in a large flask, and filtered boiling into eight or ten small flasks, which were reboiled, covered with watch glasses or beakers, and carried on a tray into the open air. The same experiment was repeated on a number of these solutions of the same strength.

The points to which this experimental inquiry tended are included in the four following propositions :—

I. That a supersaturated saline solution contained in a catharized flask will remain liquid so long as its free surface, or the surface in

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* " Sur la Tension superficielle des Liquides," par G. Van der Mensbrugghe, Répétiteur à l'Université de Gand. Mémoires couronnés par l'Acad. Royale de Belgique, tome xxxiv. 1869. See also Phil. Mag. for Dec. 1869 and Jan. 1870.

contact with the sides of the flask, does not undergo in one or many points a notable diminution of surface-tension.

II. That if we deposit on the surface of a supersaturated saline solution a drop of a liquid of feeble tension, it spreads, and crystallization takes place immediately or after a short time.

III. That while a liquid of feeble tension produces crystallization after a time more or less short, a liquid of considerable contractile force (such as pure water) not acting chemically on the solution, may be brought into contact with it without producing change of state.

IV. That as a liquid of feeble tension produces crystallization, so a solid covered more or less with a film of such liquid produces change of state, either at once or after a short time.

But before any conclusions could be drawn from the results of experiments as to the relation between the surface-tension of liquids and the state of supersaturation in saline solutions, it was necessary to measure the surface-tension of the solutions of Glauber's salt operated on. Accordingly the following data were determined, *first*, for a solution containing 1 part of salt to 1 of water, and, *secondly*, for a solution containing 2 parts of salt to 1 of water. The diameter of the capillary tube was 1·598 millim.*

Specific gravity of the solution 1 salt to 1 water at 17° C.= 1·198.

The capillary height 11 millims.

The specific gravity of the other solution = 1·289.

The capillary height 8·7 millims.

These data give, according to the formula $t = \dfrac{r \cdot h \cdot d}{2}$ (in which

t is the tension, h the height, d the density, and r the radius of the tube), for the superficial tensions of the solutions in question, not a greater value than from 4 to 5·2.

If the states of supersaturation of saline solutions depend on the maintenance of surface-tension, according to the first proposition, any force or substance that produces a notable diminution of such tension will cause the state of supersaturation to cease.

Such a force is heat, while such substances as camphor, benzoic acid, &c. have a marked effect in lowering the superficial tension of water, and in doing so undergo those remarkable gyrations which are so well known.

And first with respect to heat, applied not so as to affect the whole solution, but locally, so as to raise the temperature at one part or point of the surface, while the other parts remained at the temperature of the atmosphere.

Experiment 1. Four flasks, each about half full of a supersaturated solution of Glauber's salt (2 salt to 1 water), were exposed to a temperature of 32° F. for an hour. A red-hot poker was then passed down the neck of each flask, and in two of them the hot metal was brought into contact with the surface of the solution so as to raise a volume of vapour. There was no separation of salt in any one case.

Experiment 2. A solution containing a considerable mass of the

* The tube was calibrated by Dr. E. J. Mills, F.C.S. &c.

seven-atom salt at the bottom of the flask was moved over the flame of a spirit lamp in a line from the bottom of the flask to the neck, so as to heat one part only of the flask. The only effect was to convert a portion of the surface of the seven-atom salt into the anhydrous; but there was no crystallization. After some hours the anhydrous portion had again taken up its water of crystallization.

Experiment 3. A solution of 2 salt to 1 water that had been in the open air during twenty-four hours was uncovered, and water nearly boiling was dropped upon it. A slight cloudiness came over the solution, but there was no crystallization.

Next day a very weak solution of Glauber's salt nearly boiling was dropped upon the surface, with no nuclear action.

Experiment 4. An eight-ounce globular flask had the globe filled with a solution of 2 salt to 1 water. Solutions of two different strengths, namely 1 salt to 1 water, and 3 salt to 1 water, at a nearly boiling temperature, were dropped upon it, but with no nuclear action.

Experiment 5. A solution of 1 salt to 1 water had filtered into it a nearly boiling solution of 3 salt to 1 water. The drops descended to the bottom of the flask in beautiful rolling rings, but there was no nuclear action.

Experiment 6. The neck of a flask was inclined over the flame of a spirit-lamp, so as to boil the upper part of the solution, while the lower part remained cold. Water was driven off in vapour, so as to leave a crust of salt in the neck. This, when the flask was left to itself, gradually absorbed moisture and trickled down, and was also washed down into the solution; but there was no nuclear action either from this or from the heat.

These experiments on the action of heat lead to the conclusion that, however much it may diminish the superficial tension of the solutions, it does not apparently disturb the state of supersaturation. This result may be explained by reference to the feeble tension of the solution ($=4$), and to the fact that heat locally applied does not greatly diminish it. Moreover heat tends to oppose crystallization by increasing the solubility.

Numerous experiments were tried as to the action of newly sublimed camphor and benzoic acid on the solutions. The flasks containing these bodies floating on the solutions were plugged with cotton-wool and kept for some months, during which time they were repeatedly shaken; but there was no separation of salt. The camphor and benzoic acid formed weak solutions with the supersaturated solutions; but the tension of camphorated water being $= 4.5$, and that of an aqueous solution of benzoic acid falling within the limits 4 and 5.2, the difference in tension is too small to produce a rupture of equilibrium. The same remark applies to a solution of soap and of bicarbonate of soda, which had no nuclear action.

Action of Vapours.—It has been shown by recent researches that the presence of vapours in the air of a room, even in minute quantity, has a marked influence in lowering the tension of water and other liquids, so as to account for the discordant values of various careful measurements of the capillary heights of such liquids. As to

the nuclear action of the vapours of certain volatile liquids upon supersaturated saline solutions, many observations had been made by one of us, leading to the conclusion that such vapours are strongly nuclear when they become condensed into the form of films on the surface of the solutions, as when the latter is of a lower temperature than the former. In order to ascertain whether vapours as such (that is, without forming films) have any nuclear action, the following experiments were contrived. The vapour was presented to the surface of the solution by means of a bit of sponge tied to the end of a glass rod, wetted with the volatile liquid and carefully passed down the neck of each flask, so as to avoid touching the side, and bringing the sponge close upon the surface, avoiding also touching that*. The sponge was held over the solution several minutes, then carefully withdrawn and the flasks covered, leaving the interior charged with vapour. The liquids used were ether, absolute alcohol, chloroform, bisulphide of carbon, wood-spirit, and benzole. The solutions were of all three strengths, and the temperature from 40° to 47° F. After many hours, and even days, the flasks had a strong odour of the vapours in question ; but there was no separation of salt.

Vapour of camphor was also tried in the following manner :—

Experiment 7. A quantity of camphor was placed in a small retort, the beak of which, made chemically clean by being heated in the flame of a spirit-lamp, was passed into a flask containing a solution of 2 parts salt to 1 of water. The camphor in the belly of the retort was then boiled so as to produce a powerful jet of vapour upon the surface of the solution. The camphor condensed upon such surface in the form of a fine white powder without any nuclear action.

In this case a portion of the vapour of camphor or of the powder would dissolve in the solution without producing in it a notable diminution of surface-tension. The same remark applies to the other vapours, to the action of solid camphor and benzoic acid, of heat, &c.

So also, as stated in Part II., glycerine mingles with the solution without any nuclear action. Now the surface-tension of glycerine $=4 \cdot 2$; so that it can have no effect in lowering the surface-tension of a solution $=4$, and does not sufficiently lower the tension of a solution $=5 \cdot 2$ to produce a rupture of equilibrium.

It was also stated that bisulphide of carbon $t = 3 \cdot 3$ to $3 \cdot 5$, and chloroform $= 2 \cdot 98$ to $3 \cdot 12$, formed lenses on the surface of the solution, and that on gently agitating the flask they fell to the bottom, where they remained permanently without any nuclear action. Creosote ($=3$) behaves in the same manner. Now, in any one of these cases, the tension $t + c$ must be greater than $4 \cdot 5$, and hence there can be no separation of the salt.

We now pass on to consider the second proposition—namely, that if on the surface of a supersaturated saline solution there be depo-

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* In a few cases the wet sponge did touch the solution for an instant, so as to take up a small portion, which immediately crystallized upon the sponge; but the crystallization thus produced not being in contact with the solution, the latter retained its liquid state.

sited a drop of a liquid of feeble tension, the drop spreads and crystallization is determined. Now it is shown in Part II. that drops of ether, of alcohol, and of similar volatile liquids, as well as of certain oils, both volatile and fixed, spread over the surface of the solutions and act as powerful nuclei. On the surface-tension theory, a liquid such as ether, of which the tension$=1\cdot88$, or alcohol $=2\cdot5$, or wood-naphtha $=2\cdot11$, or oil of lavender $=2\cdot9$, must spread on the surface of a supersaturated solution of Glauber's salt of which the surface-tension is as high as from 4 to $5\cdot2$. This is true in a large number of cases that have been observed; and so far the phenomena are consistent with the theory; but there are cases in which liquids of low tension, such as oil of turpentine$=2\cdot2$ to $2\cdot4$, and some varieties of castor-oil$=2\cdot5$, do not form films, but well-shaped lenses, and remain as such during many hours, and even days. Quincke seems to have met with cases of this sort in his elaborate inquiry on the capillary phenomena of the common surface of two liquids[*]; and he endeavours to account for these exceptions to the general law by the statement that if a lens-shaped drop of a liquid 2 (of low tension) remain on the free surface of a liquid 1 (of much higher tension) without spreading itself out, then it is certain that in most, and probable that in all cases the free surface of liquid 1 is rendered impure by a thin layer of a foreign liquid 3. Now in experiments on supersaturated saline solutions the flasks, the filtering-apparatus, and the solutions must be, as already explained by one of us, chemically clean; so that in boiling and filtering a solution into clean flasks in which it is boiled up again, covered over, and left to cool in the open air of the country, it is difficult to imagine the existence of such a film as M. Quincke refers to. Moreover, did such a film exist, the solution in cooling would probably become solid under its action. Indeed this sometimes happens in the case of flasks that have been already used in experiments on the nuclear action of oils; for, however carefully they are cleansed, it may happen that one or two out of a dozen may not be quite clean, so that, in the cooling of a boiling solution, a film detached from the walls of the flask may spread over the surface with nuclear action. In order, if possible, to prevent the formation of such a film, the following experiment was made :—

Experiment 8. A solution of 1 part of Glauber's salt to 1 of water, with the addition of a bit of caustic potash, was boiled and filtered into four clean flasks. When cold, a drop of castor-oil was deposited upon the surface of each of the solutions. It flattened at first, but soon recovered the lenticular form. There was no nuclear action during an hour. On gently shaking the flasks, the oil was diffused through the solution without nuclear action.

In an experiment described in Part II. fragments of stearine were scraped into a solution with immediate nuclear action. In such a case, the stearine furnished the film-forming material that produced the solidification of the solution. The solution was boiled with the stearine in it; and in cooling the stearine formed into solid disks without nuclear action, although the flask was frequently shaken.

[*] Poggendorff's Annalen, vol. cxxxix. See also Phil. Mag. for April 1871.

In this case the boiling solution had saponified or otherwise removed the film-forming matter, or, in other words, had made the stearine chemically clean.

There is also a difficulty in the case of oil of turpentine, as in the following experiment :—

Experiment 9. A drop of an old but clear and bright oil of turpentine was deposited on the surface of a solution containing 2 parts of salt to 1 of water. The drop flashed out into a film, and the solution immediately became solid. The turpentine was now distilled, and a drop of the distillate was deposited on a similar solution, when it formed a well-shaped lens with no nuclear action, although the flasks were left out during several days.

Now the tension of the old oil first used is $=2.2$; and had the effect of distillation been greatly to exalt the tension, the experiment would have been intelligible according to the theory; but on measuring it the tension was found to be only 2.4.

A somewhat similar case is given in Part II., in which an old oil of bitter almonds was strongly nuclear, while the same oil freshly distilled had no such action, but became converted into benzoic acid, still without any separation of salt. After some days, to prove that the solution was still supersaturated, it was touched with an unclean wire and it immediately became solid.

Still, however, there are such a large number of cases in which oils and other liquids spread upon the surface of the solutions with nuclear action as to justify the labour bestowed upon the theory by one of us during the last six months. Many of these cases are stated in Part II.; but a few of them may be repeated here for the sake of comparing the action of such liquids upon solutions of different strengths, which was not done before.

If we take a number of oils, the tension of which varies from about 2.5 to 3.5, a drop of any one of them, according to the theory, ought to spread on the surface of a solution where $t = 5.2$, and not in all cases spread on the solution of which $t = 4$.

Experiment 10. Twelve flasks, containing a solution of 1 part salt to 1 of water were prepared, and a drop of each of the following oils formed films with immediate crystallization of the solutions, viz. pale seal-oil, sperm-oil, cotton-seed oil, and niger-oil. A drop of linseed-oil formed a lens; but this soon becoming ragged, crystals diverged from it. A drop of castor-oil formed a lens with no nuclear action.

Experiment 11. Three of the above solidified solutions were heated over a lamp, boiled, and covered over. The oil collected on the surface in innumerable small disks. Next morning one of the solutions was found crystallized, and the other two became solid on gently agitating the flasks.

In this case as the solutions cooled down or were gently agitated the disks spread out into films with nuclear action.

Experiment 12. A solution of 3 parts salt to 1 of water was filtered into twelve flasks, when a drop of each of the following oils deposited on the surfaces of the solutions became lenticular without

any separation of salt, viz. pale seal-oil, olive-oil, rape, castor-oil, croton-oil, niger, sperm, and cotton-seed oil.

So far this result is in accordance with the theory.

Experiment 13. A solution of the same strength as in the last experiment was employed, when a drop of seal-oil, sperm, cotton-seed, and niger spread out into films with powerful nuclear action. Linseed- and castor-oil formed lenses with no such action.

Now it must be remarked that on the day when Exp. 12 was made the weather was dull, damp, and cloudy, and during the time of Exp. 13 the weather was bright and clear. Some years ago it was a matter of frequent observation to one of us, that the formation of co-hesion-figures on the surface of water was much more rapid and de-cisive, with altogether finer and sharper results, in bright weather as compared with dull, damp, wet, or foggy weather. The same remark applies to the motions of camphor on water, and to those curious phenomena known as "camphor-currents" and "camphor-pulsations"[*]. In the production of all these phenomena, as has been shown by one of us[†], surface-tension plays a most important part; and such tension is lowered in dull foggy weather probably by the condensation of the vapour of volatile matters contained in the atmosphere. A drop of a liquid under such conditions may not spread on the surface of water or of mercury, the latter being espe-cially liable to such influences; whereas on a bright day such sur-faces are particularly active, and experiments succeed which some hours or days before failed to produce the results expected.

Then, again, as pointed out by one of us in Part II., the viscosity of the surface, or of the drop of liquid placed upon it, may greatly interfere with the operation of the law by which a liquid B spreads upon the surface A. A supersaturated saline solution has a consi-derable viscosity of surface, which it retains for many hours after it has cooled down. In the course of about twenty-four hours the more watery particles come up to the surface and the tension im-proves; so that the same surface which may have sufficient tensile force to cause a drop of oil to spread upon it, might some hours earlier have retained it in the lenticular form[‡].

There are also certain modifications to which oils &c. are subject in consequence of the presence of ozone and other matters in the air, which may somewhat disturb the results expected to be obtained from the action of surface-tension.

It was stated in Part II. that when an oil &c. assumes the len-ticular form, the solution may be agitated so as to break up the lens

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[*] Phil. Mag. for Dec. 1869.

[†] Sur la Tension superficielle des Liquides, par G. Van der Mensbrugghe.

[‡] Some of the distinguished physicists who are now engaged in studying the phenomena of surface-tension refer to the embarrassing effects of surface-vis-cosity. Thus Herr Lüdtge remarks that a solution of soap ($t = 2.8$ to 3) does not spread upon a solution of Panama-wood ($t = 5.7$); and it has been shown by one of us that the viscosity of the surface explains why a solution of soap does not spread on a solution of saponine or of albumen; and, on the other hand, the liquid drop being viscous, there is no extension, or only a feeble one, since the slight difference in tension is equilibrated by the resistance of the viscous liquid.

into a multitude of globules, and give the solution the appearance of an emulsion. In such a case the tensions of the two liquids are of nearly the same value; if not, the agitation often produces crystallization; but even in the former case it was stated that a sudden jerk will sometimes produce immediate solidification of the solution. Now, taking the tension of the solution at 5·2, and that of oil of olives at 3·7, and the tension at the surface of separation of the solution and the oil-lens at about 2, then the sum 3·7 + 2 is equal to the tension of the solution, and the spreading on the surface ought to be impossible, unless fine clear weather, absolutely clean vessels and solutions, and the absence of surface-viscosity concur to increase the surface-tension of the solution. At the surface of separation of the solution and of the glass, spreading may be possible in the case of certain oils without these concurring circumstances. Suppose a drop or a minute globule of oil to be brought into direct contact with the wet solid side of the solution, as by the jerk above referred to, the film of solution is displaced and the oil can wet the solid side. It may happen that the tension t of the solution at the wall of the flask is greater than the sum of the tension t of the surface of separation of the solution and of the oil plus the tension of the oil in contact with the solid side; that being the case, the instant solidification consequent on the jerk is accounted for.

It will be seen, then, that when the drop of oil &c. remains as a lens on the surface, there is a diminution of tension at the surface of the solution in contact with the oil; but in such a case the tension is not sufficiently lowered at one point to render molecular equilibrium impossible at this point and so break up the whole system of supersaturation. But if the solution be agitated, so as to bring into contact with the surface of the glass a portion of the drop, there will still be diminution of tension at the surface of the solution in contact with the solid, and now the diminution is sufficient to produce crystallization. Thus it appears that oils may act differently according as they alter the tension of the liquid freely exposed to the air, or the tension of the liquid in contact with the glass, which is not of the same value.

With respect to Proposition III. there is no difficulty. A liquid of considerable contractile force, such as pure water, produces no separation of salt in a solution of less contractile force. This explains a number of cases described in a note by one of us submitted to the Society in July last*, in which solutions exposed for hours together to heavy rain did not crystallize, unless the rain brought down a speck of soot or some unclean body that lowered the surface-tension of the solution. Indeed we know of no liquid of superior tensile force to that of the solution, and not acting chemically upon it, that has any influence in producing crystallization.

Proposition IV. also agrees with the phenomena. A glass rod or other solid, more or less smeared with a film of a liquid of low tension, when brought into contact with the solution determines crystallization by lowering the surface-tension. Such, then, is the func-

* Proc. Roy. Soc. vol. xx. p. 41.

tion of a nucleus with respect to supersaturated saline solutions. If the solid be made chemically clean, it may be plunged into the solution without altering its tension, and hence there is no separation of salt. And here it may be remarked that such a case is possible as that a crystal of the salt itself may be brought into contact with the solution without disturbing its tension, and hence be inactive. It has never been pretended that a crystal of the salt is not a good nucleus for a supersaturated solution of its own kind; all that has been stated by one of us is that, under special conditions, such a crystal may be lowered into the solution without acting as a nucleus.

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GEOLOGICAL SOCIETY.

[Continued from p. 149.]

April 10, 1872.—His Grace the Duke of Argyll, K.T., F.R.S., President, in the Chair.

The following communication was read:—

" Notice of some of the Secondary Effects of the Earthquake of the 10th January, 1869, in Cachar." Communicated by Dr. Oldham, of Calcutta, with remarks by Robert Mallet, Esq., C.E., F.R.S.

This earthquake was a severe one, being strongly felt in Calcutta, distant from the meizoseismic area about 200 miles, and far into the plain of Bengal.

The effects were examined on the spot a few weeks after the shock by Dr. Oldham, who anticipates being able to fix the position and depth of the centre of impulse by following the same methods as those first employed by Mr. Mallet with respect to the great Neapolitan earthquake of 1857.

These results have not yet been received; but Dr. Oldham has forwarded an extremely interesting letter on the circumstances of production of very large earth-fissures, and of the welling up of water from these, derived from the water-bearing ooze-bed, upon which reposed the deep clay-beds in which the fissures were formed.

Dr. Oldham rightly views all these fissures, which were all nearly parallel to and not far distant from the steep river-banks, as " secondary effects," and not due to fractures produced by the direct passage of the wave of shock. He also shows that the welling up or overflowing of the water in the fissures was a secondary effect also, and negatives the notion entertained on the spot of mud-volcanoes &c. having originated at those fissures.

The chief aim of Mr. Mallet's remarks was to point out the importance to geologists of rightly comprehending the dynamics of production of these phenomena, and to show that the older notions of geologists as to earthquake-fissures are untenable. He explained clearly, aided by diagrams, the train of forces by which the elastic wave of shock, on passing out of the deep clay-beds where these have a *free side* forming the steep river-banks, dislodges certain portions and throws them off towards that free side—and that this is but a case of the general law in accordance with which such elastic

waves behave towards more or less incoherent deposits reposing on inclined or on level beds, under various conditions.

Mr. Mallet also explained the dynamic conditions under which the water from water-bearing beds, such as that of ooze beneath the Cachar clay-beds, becomes elevated in the fissures formed, and gave approximate expressions for the minimum height to which the water can rise in relation to the velocity of the elastic wave particle. The paper concluded with some explanatory remarks upon the continual noises, like the irregular fire of distant artillery, heard long after the shock had passed, and when the country had become perfectly quiescent.

The noble collection of photographs which were made by Dr. Oldham, and forwarded to Mr. Mallet, illustrative of the physical features of the huge earth-fissures and other effects of this earthquake, were exhibited to the Fellows present, and are well worthy of attentive study.

XXVIII. *Intelligence and Miscellaneous Articles.*

ON THE INFLUENCE OF PRESSURE IN THE PHENOMENA OF ENDOSMOSE AND EXOSMOSE. BY M. BECQUEREL.

THE various causes to which the phenomena of endosmose, exosmose, diffusion, and dialysis are due have been the object of the important researches of Dutrochet, Graham, Liebig, and other physicists and chemists, who have determined the part that each of them contributes in the production of the phenomena observed; but they have not taken into consideration all the conditions which intervene in that production, especially the following:—(1) the pressure which acts as soon as endosmose has raised the level of one of the liquids above that of the other, whence results a filtration, through the separating film, of the most pressed liquid towards that which is less so, the effects of which appear to be subject to very simple laws, as we shall see; (2) the formation of an insoluble compound by the reaction of the two liquids upon each other when this takes place, a case which had not yet been examined; (3) the action of the electrocapillary currents resulting from the same reaction, which I have already brought before the Academy in several memoirs.

I commence by giving a very succinct analysis of the researches of Dutrochet and Graham, as well as of those of Magnus and Liebig, on endosmose, in order the better to establish the relation of the effects which they observed to those about to be considered, relative to the influence of pressure on the filtration which takes place through a capillary film—an influence which makes itself perceptible in the phenomena of endosmose and exosmose, as well as in the effects resulting from the circulation of liquids in the tissues of living bodies, especially of the blood in the arteries and veins. Two apparatus were set up to exert pressures up to 2500 millims. of water or another liquid, and were provided with a cathetometer which permitted the determination of the height of the liquid columns

with great accuracy. For diaphragm I took successively parchment paper, bladder, and a porous vessel of unglazed porcelain. The results obtained show that the ratios between the quantities of liquid which pass through and the mean pressures are constant ; that is, whatever be the liquid, the quantity which passes through in a given time is proportional to the mean pressure, alcohol not excepted.

I have found that, during the filtration, the outside of the porous vessel becomes covered with bubbles proceeding from the air contained in the water and in the diaphragm, which is disengaged in passing through the latter in consequence of the capillary affinity exerted by it upon the liquid. These bubbles, more or less obstructing the pores, render the filtration irregular—an inconvenience which is in great part got rid of by working with distilled water from which the air is expelled by boiling. But this is not the only error to be avoided ; the inequality of calibre in different parts of the length of the tube is another for which allowance must be made. These two causes are sufficient to account for the differences which have been observed.

The following coefficients were found to express the ratios between the mean pressures and the quantities of liquid filtered through a porous porcelain diaphragm in half an hour with the same filtering surface :—

Hydrochloric acid . . . . 0·187
Distilled water . . . . 0·165 } increased by their
Ammonia . . . . 0·139 } volume of water.
Chloride of calcium in solution at 35° 0·055.

The experiments were made at temperatures varying from 15° to 20°. It is to be remarked that, when experimenting with bladder, after a certain number of hours the cells are distended and the coefficients progressively increase until they become double ; with vessels of porous porcelain nothing of the kind happens ; the course of the flow is regular.

Other solutions were also submitted to experiment ; but in this abstract I shall only mention the results obtained with a solution of sulphate of soda and another of sulphate of lime, both concentrated and deprived of air, and giving rise by their reaction to an insoluble crystallized precipitate.

I have shown, in a previous memoir, that when a solution of nitrate of lime is introduced into a tube closed at the lower end with parchment paper, and the tube dips into a test-glass containing a solution of sulphate of soda, we soon see form on the surface of the diaphragm in contact with the sulphate solution, and also in that solution, a very great number of fistular stalactites of crystallized sulphate of lime, which little by little lengthen until they reach the bottom of the test-glass, where the substance spreads out.

I then worked in the opposite direction: I poured into the tube containing the liquid exerting the pressure a solution of sulphate of soda, and the nitrate-of-lime solution into the test-glass, in order to

force the sulphate through the diaphragm; I did not succeed in doing so. In seven hours the column was lowered one sixth; but, what is remarkable, the stalactites were not formed; merely a not very thick and pretty compact layer of crystals of sulphate of lime was deposited on the surface in contact with the sulphate solution. The pressure therefore hindered the formation of the stalactites and the passage of the sulphate solution into that of the nitrate.

Some filtration-experiments were also made with defibrinated blood under 150 millims. pressure of mercury, equal to that of the blood in the arteries; the filtrate consisted of serum only.

It is to be presumed that in the arteries, under the pressure of 150 millims. to which the blood is subject, there must be an infiltration of serosity through the walls of the arteries in quantities proportional to the variations of the pressure. It has been said above, that the flow through the organic membrane distended the cells; under the empire of life nothing like this should take place, at least in the normal state; equilibrium in the constitution of the vessels must be constantly maintained.

We have now some conception of the possible origin of exosmose: it is due partly to diffusion, and partly to the filtration resulting from the pressure of the column of liquid resulting from endosmose. I conceive that it was hardly sensible in the experiments of Graham, who operated with large surfaces affording but little elevation to the liquid columns resulting from the endosmose. Dutrochet was right in saying that exosmose transports more of salts than endosmose, as exosmose results partly from pressure, which causes the liquid to filter through with the salt which it holds in solution.—*Comptes Rendus de l'Acad. des Sciences,* July 8, 1872, pp. 50–52.

ON THE ACTION OF OZONE UPON VULCANIZED CAOUTCHOUC.
BY PROF. ARTHUR W. WRIGHT.

In using the Holtz's electro-machine, in the summer season it is often very difficult to make it retain any considerable charge, or even to keep up its action for more than a few minutes. The ebonite insulators are found to have lost in a large degree their insulating power, and to have become conductors to such an extent that considerable sparks may be drawn from them at points several inches distant from the metal parts supported by them, thus dissipating the greater portion of the charge. This is the usual condition of things when the machine, after much use, has stood for some weeks in the warmer portion of the year unused. The surface of the ebonite becomes hygroscopic, condensing upon itself a large amount of moisture, the accumulated liquid being sometimes so abundant as to trickle down in drops.

Having noticed on one occasion that this liquid had an acid taste, I was led to examine it more closely; and the ordinary tests very speedily showed it to be sulphuric acid. Its presence was a sufficient explanation of the defective insulation. Similar deposits of moisture were found upon the ebonite jackets of two induction-coils some time after they had been used.

As nothing containing sulphur had been used about the apparatus, the acid was evidently derived from the ebonite itself. The first thought was that the material had been heated in the process of vulcanization sufficiently to oxidize the sulphur; but as the sulphurous oxide, if thus formed, would be dissipated by the heat, this could hardly be regarded as the source of the sulphuric acid, especially as the latter did not appear until after the apparatus had been used. It is well known that vulcanized caoutchouc is affected by ozone, and that the ordinary rubber tubes through which it is passed are attacked and quickly perforated by it. It seemed most probable, then, that the acid was the result of the action of the ozone upon the insulators; and experiments were made which entirely confirmed this supposition.

To the exit-tube of the ozonizing apparatus described in the Phil. Mag. for August (p. 156) was attached one end of a vulcanized rubber tube a few inches long, the other end being slipped upon the glass tube of a small wash-bottle containing some thirty or forty cubic centimetres of water. Air was slowly driven through the apparatus, and, having been strongly ozonized by the action of the electricity, bubbled up through the water. This was continued for an hour and a half. At the end of this time common air was passed through the apparatus to displace the ozone left in it, the tubes were removed, and the bottle closed with a glass stopper. On opening the bottle some time afterward, there was an unmistakable odour of sulphurous oxide, and the water reddened blue litmus paper very quickly and strongly. A strip of litmus paper, hung in the bottle so as not to touch the water, was completely reddened in a short time; and this happened even after several days had elapsed from the time of the experiment. The water tested with chloride of barium gave a considerable crystalline precipitate, leaving no doubt of the presence of sulphuric acid.

A small slip was cut from a thin plate of ebonite, cleaned and dried, and placed in a small bottle, into which ozone was driven as before. In a short time it was bedewed with moisture having an acid taste, and exhibiting the same properties as that found upon the insulators of the machine.

In order to determine whether the sulphur itself could be directly oxidized by ozone, a quantity of fine flowers of sulphur was gently rubbed into a loose lock of dry cotton, so as to diffuse it as much as possible. The cotton was placed in a dry wash-bottle, and connected by means of a glass tube with a second wash-bottle containing a little water, all the connecting tubes being of glass. Ozone was passed through the bottles for an hour and a half; but at the end of this time not the slightest evidence of any action upon the sulphur could be detected. This was what might have been expected; for as the air often contains a small percentage of ozone, sulphur exposed to it would undergo slow alteration, with loss of weight; and it does not appear that any thing of the kind has ever been observed.

It is evident that while the ebonite is undergoing decomposition by the ozone, the oxygen combines with the issuing sulphur to form sulphurous oxide, which with the atmospheric moisture produces

sulphurous acid, this in turn being converted into sulphuric acid by the further action of the ozone. The absorption of moisture from the atmosphere by the sulphuric acid produces the dew-like deposits observed.

The deleterious effect upon the insulators can be remedied by neutralizing the acid with some substance which will not form a hygroscopic compound or essentially lessen the insulating power of the ebonite. I have used oxide and carbonate of magnesium with very good effect. A little of either of these substances in fine powder is sprinkled upon a soft cloth or piece of chamois leather and rubbed over the insulators. The excess is removed with a wet cloth, and the surface, after drying, cleaned and polished by rubbing with a soft woollen cloth very slightly moistened with carbonic disulphide. As the ebonite is attacked by the latter substance, care should be observed, in employing it, to use only so much as is needed to facilitate the polishing process without injuring the surface. The ebonite may be somewhat discoloured by these operations; but the colour can be restored by rubbing with a little oil, or will return of itself after a time.

Probably a better method may be found; but this gives very good results. On one occasion, early last autumn, when the electro-machine had not been used for some months, the sparks obtained on charging it and using small condensers were only about one quarter of an inch in length, and the action of the apparatus was very feeble. The insulators were quite damp with the accumulated moisture. When this had been removed by the process described, sparks eight or nine inches in length were obtained at once, and the machine worked with nearly its usual energy.—Silliman's *American Journal* for July 1872.

ON THE INSTANTANEOUS OXIDATION OF ALCOHOL.
BY M. A. HOUZEAU.

Here is a simple example of the direct conversion of alcohol into acetic acid and aldehyde, without the cooperation of any other agent than oxygen modified by electricity.

If, into a bottle of half a litre capacity filled with concentrated moist ozone obtained by means of one of my single- or double-acting ozonizers, about 10 cubic centims. of absolute or hydrated alcohol be poured, a strong agitation of the bottle for a few seconds is sufficient to cause the neutral and almost inodorous alcohol to manifest a strong acid reaction with litmus paper, due to the acetic acid formed[*], and exhales an odour of aldehyde, the presence of which is demonstrated by the reducing action of the liquor upon an ammoniacal salt of silver. But the most curious fact of the experiment is the simultaneous formation of relatively considerable quantities of oxygenated water; a few cubic centims. of the alcoholic liquor turn the mixture of chromic acid and ether deep blue.

[*] After the action of ozone, alcohol saturated with lime-water and evaporated to dryness leaves a residuum which liberates acetic acid on contact with diluted sulphuric acid.

On operating in like manner with ordinary oxygen (that is, before the gas has undergone the obscure electrification), nothing similar is observed. Even after twenty-four hours of contact the alcohol remained neutral, inodorous, and without action upon either the salt of silver or chromic acid.

Ether, in the same circumstances, undergoes from concentrated ozone an analogous and still more rapid oxidation, attended by the production of oxygenated water.

If we compare these effects of oxidation with the similar effects upon alcohol of contact with oxidizing bodies such as chromic acid, a mixture of sulphuric acid and bichromate of potass, &c., one cannot but recognize the profound analogy which seems to exist between free ozone and oxygen as it exists in its combinations; indeed it was this very analogy which led me long ago to suppose that ozone might be only the primitive state of oxygen.

However this may be, these experiments demonstrate that concentrated ozone (which can now be easily produced with my ozonizing tubes) is an oxidizing agent at the same time simple and energetic, the employment of which may be useful in researches of organic chemistry.

When we endeavour to calculate the real quantity of ozone contained in odoriferous oxygen from the products of the oxidation of alcohol, and compare the result with that furnished by the oxidation of either iodide of potassium or metallic silver, the numbers arrived at differ remarkably from one another, and silver gives the smallest product. Hence at present we ought not to accept without reserve the numbers expressing that quantity of ozone.

In concluding I cannot too strongly advise chemists who make use of concentrated ozone to do so with the utmost caution; breathed, even in very small quantity, it suddenly occasions inflammation of the mucous membranes, which I have known to bring on spitting of blood.—*Comptes Rendus de l'Acad. des Sciences*, July 15, 1872, pp. 142, 143.

ON SOME EFFECTS OF SLOW ACTIONS, PRODUCED IN THE COURSE OF A CERTAIN NUMBER OF YEARS. BY M. BECQUEREL.

I have already called the attention of the Academy to the effects which constitute the subject of this memoir; but I have thought it necessary to resume the question in order to develope it further, and then to show the influence of time in the effects produced.

I used, in these researches, either a cracked tube filled with a metallic solution and dipping in an alkaline solution in which a metallic oxide was dissolved, or a porous diaphragm of unglazed porcelain instead of the tube, or a glass vessel hermetically sealed, containing an acid or alkaline solution in which was immersed a mineral substance.

With an electrocapillary apparatus, a solution of gold and another of plumbate of potash gave, in the space of two years, on the one hand minium in the crystallized condition, similar to that obtained in the dry way, and, on the other, metallic gold.

Among the products obtained in consequence of slow actions in a vessel hermetically sealed during twenty years, and which have their analogues in nature, I will mention the following :—

(1) Some crystals of arragonite, formed upon a piece of gypsum shaped like a spearhead, 1 decim. in length and 1 centim. in thickness, digested in a solution of bicarbonate of potash contained in a vessel hermetically sealed; the sulphate of lime almost entirely disappeared, and there remained a thick coating of crystals of arragonite.

(2) Operating with a solution of subcarbonate produced rhombohedric crystals of carbonate of lime.

(3) A similar piece of gypsum, kept during the same time in contact with a solution of arseniate of ammonia, gave crystals of arseniate of lime, perhaps as fine as the natural ones.

(4) With a solution of aluminate of potass and gypsum, I obtained crystallized double sulphate of lime and potass, which is no other than glauberite in which soda has been replaced by potass.

(5) Pieces of galena, immersed during twenty years in a solution of bicarbonate of potass, gave well-characterized crystals of carbonate of lead belonging to the system of the right prism with rhombic base.

(6) With pieces of limestone immersed in a solution of plumbate of potass, I obtained hydrated carbonate of lead in crystalline scales with a nacreous aspect.

(7) Malachite (bibasic carbonate of copper) I had already obtained by the reaction of a solution of nitrate of copper upon limestone to change it into subnitrate, which was then digested with bicarbonate of soda to form a double carbonate, which was decomposed with a fresh solution of nitrate of copper. Working thus I obtained a crust of more or less thickness adhering to the surface of the limestone. I studied this formation again, modifying the process. The limestone was in slabs of 1 centim. thickness; and the operation took place *in vacuo*, in order that the solutions might penetrate the interior of the slabs and the gases formed there escape. By working with a solution not much concentrated, it was ascertained that the first two transformations sufficed for obtaining a slab of malachite sensibly free from lime and nitric acid, having the same grain as the limestone; the epigeny, therefore, was complete. The grains are in the crystalline state. Under high pressures the same result may probably be obtained with rather compact limestone.

The effects of the slow actions we have here considered, and which sometimes produce epigeny (that is, replacement of substances by other substances without changing either the form of the body or those of its constituent parts), I explain as follows :—

When, for example, porous limestone is digested in a concentrated solution of nitrate of copper, there results from the reaction which takes place a disengagement of carbonic acid gas and a production of nitrate of lime, which remains in solution, and of insoluble subnitrate of copper, which takes the place of the grains of limestone thus transformed into subnitrate, as the subnitrate is forced to occupy the place of the grains of limestone by the carbonic acid gas and the solution of nitrate of lime filling the pores. The gas and the solu-

tion of nitrate of lime can only issue completely from the pores by placing the transformed limestone in the vacuum of the air-pump in contact with the water and renewing it from time to time.

When the subnitrate is placed in contact with a solution of bicarbonate of soda to change it into malachite, nitrate of soda remains for a long time in the pores, which in time effloresces upon the surface of the malachite, so great is the molecular attraction exerted by the walls of the pores upon this compound.

I do not speak of the electrochemical effects which may intervene in the actions of which we have spoken, because I have already described them.

The analyses of the substances above mentioned were made by M. Guéraut, a distinguished pupil from the Laboratory for Advanced Studies, under the direction of our confrère M. Fremy, at the Museum of Natural History, whom the Minister of Public Instruction kindly placed at my disposal to aid me in my experiments.—*Comptes Rendus de l'Acad. des Sciences*, July 8, 1872, pp. 52–54.

REPLY TO PROFESSOR CLAUSIUS.   BY P. G. TAIT.

Professor Clausius has so long, and so repeatedly, claimed as his own the correct proof of the Second Law of Thermodynamics, that no one can be astonished to find him unwilling to allow that his claims are unfounded.

But I must protest against his making accusations of deliberate suppression (*Absichtlichkeit*) &c. and repeating them in the indirect and offensive form of a statement that he did not apply them to Sir W. Thomson.

There has been nothing in the language I have employed, even had it been tenfold more pointed, which is not admissible in fair and temperate discussion.   I have made no charges (though strongly tempted to do so by Professor Clausius's first letter), I have simply examined historical facts and given what appears to me to be the natural and inevitable conclusion from them.   But, after having taken every precaution to insure accuracy, to be first accused of deliberate suppression, and then to be told that the tone of my far too mild reply renders it impossible for Professor Clausius to continue the discussion, is a trifle too much.

In common with all the scientific friends I have consulted, I am unable to perceive that Professor Clausius has " refuted " any one of my former remarks, or that he is likely to be able to refute any of the others—though he says it can be easily done.   Let Professor Clausius attempt the refutation, if he thinks proper to do so : but in future it is to be hoped he will leave offensive and unjust charges unmade.   As I consider that my last letter contains all that it is necessary for the present to say for my own view of the matter, I shall continue to maintain and to promulgate the opinions therein expressed, until convinced by argument, not by personalities, that they are incorrect or insufficient.

XXIX. *On the Cooling of Gases.*
By MM. Jamin and Richard[*].

DULONG and Petit, in their celebrated work on the laws of cooling, studied first the effect of a vacuum. They found that the velocity of cooling of a thermometer in the centre is expressed by the formula

$$v = ma^\theta(a^t - 1).$$

a is an invariable quantity, m a coefficient proportional to the surface and the emissive power of the thermometer, $t + \theta$ and θ the temperatures of the thermometer and the enclosure.

No objection can be made to this law, which has moreover been confirmed by the admirable researches of De la Provostaye and Desains.

The second part of the work, which is devoted to the study of gases, is not so unexceptionable. Dulong and Petit, remarking that in this case the diminution of temperature is more rapid, assume :—(1) that the radiation persists without alteration, just as if it took place *in vacuo*; (2) that the augmentation of the observed effect represents the cooling-power of the gas. They then measure the total velocity of cooling, V, from which they subtract v (that which would take place *in vacuo* for the same values of θ and t), and find that the remainder v' satisfies the relation

$$v' = n\mathrm{HC}t^{1\cdot233}.$$

It is sensibly equal to $0\cdot5$; and n is a coefficient which de-

pends only on the gas—very small for carbonic acid, greater for air, and very considerable for hydrogen. It is assumed that v' measures the effect of the gas.

In reasoning thus, Dulong and Petit make a pure hypothesis, and probably commit an error. It is always possible to represent the total velocity V by the sum $v + v'$; but it is not proved that v expresses the radiation as it exists in the gas, and v' the cooling due to the gas itself. On the contrary, it is probable that the radiation is less than v, since the gas is imperfectly diathermanous, as Tyndall has proved, and consequently that the effect attributable to the gas must be augmented by so much.

Besides, Dulong and Petit appear to have taken no account of the kind of action exerted by the gas; at least they do not attempt to explain it. They present that action as a fact; they assume that the gas remains at the temperature of the enclosure, that the presence of the thermometer changes neither its temperature nor its pressure. The apparatus they employ would not permit the ascertaining of this change, if it took place. Thus they overlooked the true conditions of the problem.

In truth, the gas is warmed, and its pressure increases. Let us take a glass balloon immersed in water, furnished with a sensitive manometer, and traversed along one of its diameters by a fine and resisting platinum wire. As soon as it is heated by an electric current, we shall see the manometer rise progressively and the temperature of the gas increase. One of us, in a previous study, even ascertained that the heating was much greater in proportion as the pressure and volume were less.

This observation explains to us, in the first place, the perturbations discovered by MM. de la Provostaye and Desains in the case of very small enclosures and feeble pressures. The gas being then very much heated, its temperature can no longer be confounded with θ (that of the enclosure) and the excess t measured by the difference between the degree of the thermometer and θ. The real excess is smaller; the factor t^d must be diminished, either by replacing t by its true value and diminishing d, or by attributing to d decreasing values variable with the pressure H.

But this fact has a still greater importance in that it clearly reveals the part played by the gas during the cooling. It is heated by contact with the thermometer, and transmits the heat to the exterior covering, which absorbs it. At the first instant it receives more than it gives up; and the manometer rises progressively with a decreasing velocity, then remains stationary when the heat taken up by the thermometer is equal to that which it yields to the enclosure. The gas, therefore, acts as a conducting mass, being heated on one side, cooled on the other,

serving as a vehicle for the heat, and being in unstable equilibrium between the gain and the loss.

But its mode of conductivity is quite special. Let us in thought divide the gaseous mass into two concentric equal parts by an impermeable partition placed between the thermometer and the enclosure. We can imagine the interior mass alone being heated 2τ, taking an excess of pressure $2h$. If we open the partition, the pressure and the temperature will fall τ and h in this mass, but will rise as much in the part exterior to the partition; both will then have the same pressure and the same heating. This reasoning may be repeated by multiplying the partitions; and on passing afterwards to continuity, we find that the heat is transmitted from the thermometer as far as the enclosing boundary with and by the transmission of the pressures, and that the temperature is the same at every point. But a thermometer placed at one point will not give this temperature; for it will receive and absorb the heat radiated through the gas, of which it will receive so much less as it is nearer one side of the vessel.

It will be remarked that, the transmission of the pressure being instantaneous, it will be the same with the propagation of the heat from the centre to the exterior—and that if different gases become heated or cooled more or less rapidly, this can only depend on the greater or less rapidity with which they take heat from a heated solid surface or give it up to the wall of the vessel which contains them. In short, gases have an instantaneous internal conductivity, and place themselves in equilibrium of temperature and pressure. These conclusions, however, suppose that they are diathermanous.

The part played by gases being so well defined, it will be conceived how their cooling-power can be deduced. Let us replace Dulong's thermometer by a wire heated by means of an electric current to an excess of temperature t. It will lose during each unit of time a quantity of heat equal to $-\mathrm{P}c\dfrac{dt}{dx}$ and give it up to the gas. This will take excesses of temperature and pressure $d\theta$ and h, and transfer to its envelope the quantity of heat $pc\dfrac{d\theta}{dx}$. When the stationary condition is attained, this gain and this loss will be equal, and we shall have

$$-\mathrm{P}c\frac{dt}{dx} = pc\frac{d\delta\theta}{dx}. \quad\ldots\quad (1)$$

On the other hand, the heat given up by the gas to the enclosure is proportional to the surface of contact s, to a factor which will be special for each gas, and to a function of H and

of h; we shall therefore have

$$pc\frac{d\delta\theta}{dx}=s\mathrm{K}f(\mathrm{H},h). \quad . \quad . \quad . \quad . \quad (2)$$

If, then, we knew $f(\mathrm{H}, h)$—that is to say, if we knew the law according to which the velocity of cooling $\dfrac{d\delta\theta}{dx}$ of a heated gas varies, $\dfrac{dt}{dx}$ could be calculated by means of equation (1), and we could measure directly and without hypothesis the cooling-power of the gas. The two questions are connected and equally interesting. We shall therefore divide this memoir into two parts, and, 1st, study the cooling of heated gases within an enclosure; 2ndly, measure the heat given up to these same gases by a heated solid placed in their midst.

Part I. *Laws of the Cooling of Gases.*

The apparatus consists of a large balloon of glass, 32 centims. in diameter. It is immersed in a vat at θ degrees filled with water which is continually agitated by a current of air, and connected with a mercurial manometer, which, observed by means of a cathetometer, gives the initial pressure H. The balloon is accompanied by a bottle, which shares its temperature; the two communicate by a three-way cock, through which a vacuum can be produced in both or a gas introduced at the same pressure. When this is done, the communication is closed, and a second is opened through a differential water-manometer, one of the two branches of which communicates with the bottle, the other with the balloon. The heights are equal when the temperatures are the same; but if we heat the gas in the balloon, it assumes an excess of pressure h, which the manometer measures with great delicacy, since it contains water, whatever the initial pressure H.

The balloon is traversed along one of its horizontal diameters by a platinum spiral of great resistance; this is a focus, of negligible mass, which developes, by means of an electric current, a known quantity of heat, of which one part traverses the enclosure by radiation, while the other heats the gas. The wire is heated to redness; and when the manometer has attained its stationary condition, the circuit is broken. The spiral is extinguished immediately; at the end of ten seconds it is entirely cooled; and from that moment h diminishes regularly with decreasing rapidity. Then, while an assistant counts the time aloud, at the end of every five seconds the observer reads the value of h, which is written down by a third person. Use readily familiarizes this kind of observation; and more precision

is attainable than could be supposed. One then constructs the curves of the values of h, taking the times for abscissæ. They differ from one another; and it is immediately seen that the cooling is as much more rapid as the pressure H is less.

These curves express the phenomenon graphically; we must now find the equation. We thought at first that, the excess of pressure h being very small, Newton's law $h = M e^{-ax}$ could be applied. Indeed it differs very little from experience, which it represents well enough for a time not very great; but it cannot embrace the whole of the observations. We made the comparison by three operations. First we took, upon the curve, abscissæ in arithmetical progression: the successive ordinates were to be in a constant ratio; they were not found to be exactly so. Then we drew tangents which should satisfy the condition

$$-\frac{dh}{dx} = ah \log e;$$

that is to say, $\dfrac{dh}{dx}$ ought to represent the ordinates of a straight line of which h would be the abscissa; but we ascertained that the line thus constructed had a parabolic form. The third and best process is the following. Newton's law gives

$$\log h = \log m - ax \log e,$$

which is the equation of a straight line forming with the axis of x an angle whose tangent is $-a \log e$. In reality the line is curved; Newton's law must therefore be rejected.

We then thought to express by Dulong's formula the heat which a gas yields to its envelope, which regulates that which the gas takes from the central thermometer, and which would be

$$-\frac{dh}{dx} = n' H^{c'} d\theta^{d'}, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (3)$$

or else, replacing $d\theta$ by its value as a function of h, which is $\dfrac{h}{\alpha H}$,

$$-\frac{dh}{dx} = \frac{n'}{\alpha^{d'}} \frac{h^{d'}}{H^{d'-c'}} = m \frac{h^{d'}}{H^{d'-c'}}, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (4)$$

or else, finally, taking the logarithms of both sides,

$$\log\left(-\frac{dh}{dx}\right) = \log m + d' \log h - (d'-c') \log H. \quad \cdot \quad (5)$$

Under this form equation (5) has been completely verified; it signifies :—

(1) That, for any constant values of the pressure H, $\log\left(-\dfrac{dh}{dx}\right)$ represents the ordinates of a first system of straight lines, all parallel, whose abscissæ are $\log h$, and which make with the axis

of the abscissæ an angle the tangent of which is equal to the exponent d';

(2) That, for any constant values of the excess of pressure h, the values of $\log\left(-\dfrac{dh}{dx}\right)$ are represented by the ordinates of a second system of parallel straight lines whose abscissæ are $\log H$, and which make with the axis of the abscissæ an angle the tangent of which is equal to the exponent $d' - c'$.

We will indicate how the verifications were made, taking hydrogen for example.

Under pressures successively equal to 823·7 millims., 689·7 millims., ..., the value of h was observed every five seconds (as was said before); then, on one and the same sheet of ruled paper, the values of h were constructed, the time being taken for abscissa—which gave as many curves as there were series of observations; and upon these curves the points having equal ordinates h 200, 190,..., were marked by horizontal lines. These values of h are entered in the first column of the opposite Table. The tangents were drawn to these several points, and the value of $\dfrac{dh}{dx}$ obtained for each curve. They are placed in the columns marked A.

(1) The values of $-\dfrac{dh}{dx}$ in each column A correspond to one and the same value of H and to different excesses of pressure; and if, taking $\log h$ for abscissæ, we construct the line whose ordinates are $\log\left(-\dfrac{dh}{dx}\right)$, it is found to be exactly a right line. On repeating the same construction the same result is found for each of the pressures H. Moreover all these right lines are parallel, and differ only by the ordinate at the origin; they make with the axis of the abscissæ an angle the tangent of which is $= 1\cdot2$. The exponent d' is therefore equal to this number. The columns No. 1 contain the values of $-\dfrac{dh}{dx}$ derived from the several right lines opposite the observed numbers of series A. The agreement is very satisfactory.

(2) The observed values of $-\dfrac{dh}{dx}$ in any of the horizontal lines answer to equal values of the excess of pressure h and to different initial pressures H. The logarithms of $-\dfrac{dh}{dx}$ are constructed by taking for abscissæ the values of $\log H$; and as many outlines are obtained as there are horizontal lines in the Table. We find that they represent a second system of right lines, all parallel,

Hydrogen. Cooling of the gas ($n'=2\cdot69$; $d'=1\cdot20$; $c'=-0\cdot66$).

h	H=823·7 millims.			H=689·7 millims.			H=484·2 millims.			H=421·2 millims.			H=360 millims.			H=274·7 millims.		
	A. Observed	$-\dfrac{dh}{dx}$ Calculated No. 1	No. 2	A. Observed	$-\dfrac{dh}{dx}$ Calculated No. 1	No. 2	A. Observed	$-\dfrac{dh}{dx}$ Calculated No. 1	No. 2	A. Observed	$-\dfrac{dh}{dx}$ Calculated No. 1	No. 2	A. Observed	$-\dfrac{dh}{dx}$ Calculated No. 1	No. 2	A. Observed	$-\dfrac{dh}{dx}$ Calculated No. 1	No. 2
200	18·1	18·7	17·9	19·5	20·9	20·0												
190	17·2	18·5	17·1	18·3	19·5	18·8												
180	16·9	16·3	16·4	18·3	18·5	18·3	21·7	23·3	22·5									
170	15·8	15·3	15·3	16·4	17·1	16·6	21·9	21·9	20·6									
160	14·2	14·2	13·9	15·9	16·0	15·6	20·5	20·3	19·3	20·9	21·4	22·4	23·5	23·8	21·9			
150	13·4	13·3	12·7	15·0	14·9	14·3	19·3	18·8	17·9	19·8	19·8	21·1	22·2	22·3	20·4			
140	12·6	12·2	11·9	13·6	13·7	13·3	18·4	17·6	16·6	17·4	18·2	18·2	20·5	20·5	18·8			
130	11·3	11·1	11·0	12·6	12·4	12·3	16·0	15·8	15·4	16·3	16·7	16·8	18·7	18·7	17·4			
120	10·5	10·1	10·0	11·2	11·2	11·4	14·7	14·3	14·2	15·3	15·1	15·6	17·2	16·9	16·1	20·6	20·1	20·3
110	9·8	9·1	9·7	10·0	10·2	10·2	13·2	12·9	12·8	13·6	13·9	13·9	15·6	15·3	14·6	18·2	18·1	18·4
100	8·6	8·2	8·4	9·3	9·1	9·3	12·1	11·5	11·7	12·5	12·2	12·8	14·3	13·6	13·3	16·7	16·2	16·9
90	7·5	9·2	7·3	8·4	8·0	8·2	10·7	10·2	10·2	11·2	10·8	11·2	12·7	12·0	11·7	14·6	14·3	14·9
80	6·6	6·2	6·3	7·3	6·9	7·1	8·9	8·8	8·9	9·8	9·3	9·8	11·0	10·4	10·2	12·5	12·3	13·1
70	5·7	5·3	5·4	5·8	5·9	6·1	7·5	7·5	7·7	8·4	8·0	8·4	8·9	8·9	8·8	10·5	10·5	11·2
60	4·4	4·4	4·5	5·0	4·9	5·1	6·3	6·2	6·5	6·3	6·6	7·1	7·1	7·3	7·4	9·0	8·7	9·5
50	3·5	3·6	3·6	4·0	3·9	4·1	5·2	5·0	5·2	5·5	5·3	5·8	5·7	5·8	6·0	7·0	7·0	7·8
40	2·5	2·7	2·7	3·3	3·0	3·1	3·9	3·8	4·0	4·1	4·1	4·5	4·5	4·5	4·6	5·2	5·3	6·1

making with the axis of the abscissæ an angle whose tangent is -0.66, which signifies that the exponent $d'-c'$ is $=0.66$, or that $c'=0.54$. The columns No. 2 indicate the values of $-\dfrac{dh}{dx}$ derived from the right lines of the second system, opposite the observed values; and it can be seen that they are equal to them.

This happy method has therefore permitted us to verify separately the law relating to the two variables H and h, and to determine the two exponents c' and d'. The coefficient m is deduced from formula (3):

$$m = \frac{dh}{dx}\,\frac{\mathrm{H}^{d'-c'}}{h^{d'}}.$$

As many values are found as there have been observations; and the complete equality of all the results is a wholesale verification of the formula found.

Value of $m = \dfrac{n'}{\alpha^{d'}}.$

Hydrogen (pressure 689·7 millims.).

$h.$				$n'.$	$h.$				$n'.$
200	.	.	.	2·64	120	.	.	.	2·70
190	.	.	.	2·70	110	.	.	.	2·71
180	.	.	.	2·70	100	.	.	.	2·73
170	.	.	.	2·65	90	.	.	.	2·71
160	.	.	.	2·66	80	.	.	.	2·73
150	.	.	.	2·67	70	.	.	.	2·72
140	.	.	.	2·68	60	.	.	.	2·71
130	.	.	.	2·69		Mean	.		2·69

Formula (3) gives, for the velocity of diminution of pressure of heated hydrogen,

$$-\frac{dh}{dx} = \frac{n'}{\alpha^{d'}}\,\mathrm{H}^{c'}h^{d'} = 2.69\,\mathrm{H}^{0.54}h^{1.2}. \qquad . \quad . \quad . \quad (3)$$

Such is the final formula, which sums up all the observations. There only remained the extension of it to other gases, which we did for carbonic acid and air, the following being the numbers found:—

			Carbonic acid.	Air.	Hydrogen.	Mean.
$\dfrac{n'}{\alpha^{d'}}$	.	.	0·732	1·15	2·695	
c'	.	.	0·54	0·54	0·54	0·54
d'	.	.	1·15	1·13	1·20	1·16
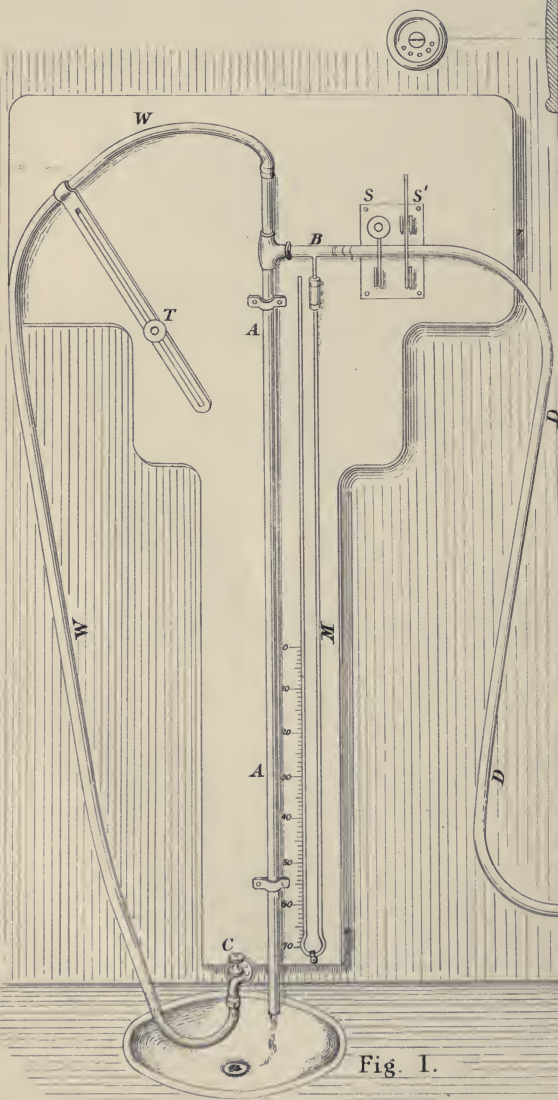
Fig. 2.

Fig. 1.
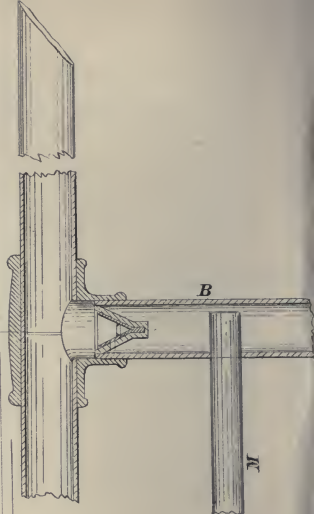
To recapitulate :—

(1) The velocity of cooling of gases $-\dfrac{dh}{dx}$ is expressed by Dulong's formula

$$-\frac{dh}{dx} = n'\mathrm{H}^{c'}\delta\theta^{d'}.$$

(2) The exponents c' and d' are the same for all gases, and sensibly equal to those found by Dulong for solid bodies.

(3) n' is different according to the gas; in our experiments it has constantly the same ratio as in Dulong's.

(4) The quantity of heat lost in the unit of time is

$$pc\frac{d\delta\theta}{dx} = \mathrm{SKH}^{c'}h^{d'}, \quad . \quad . \quad . \quad . \quad . \quad . \quad (2)$$

or, reducing,

$$-\frac{dh}{dx} = \mathrm{K}\frac{1+a\theta}{r}\mathrm{H}^{c'}\delta\theta^{d'}.$$

The coefficient n' is therefore inversely as the radius of the balloon, proportional to $(1+a\theta)$ and to a factor K characteristic of each gas.

XXX. *On an Improved form of Filter-Pump.* By T. E. THORPE, *F.R.S.E.**

[With a Plate.]

IN the *Berichte der Deutschen Chemischen Gesellschaft* (No. 7, 1872), Dr. Mendelejeff is reported to have described a new form of filter-pump devised by M. Jogno, of Moscow, which is so exceedingly simple and efficacious that it will doubtless be universally set up in laboratories. The disposition of the apparatus will be readily understood from the annexed figure (Plate III.), which represents it in the modified form about to be described. It consists of a tube A A about 1 metre in length and from 8 to 10 millimetres in diameter, to the side of which is affixed a side tube B about 5 centims. in length. The upper end of the vertical tube A is cut slantwise in the manner represented in the enlarged figure (fig. 2), and is connected by means of a strong but sufficiently elastic caoutchouc tube with the stopcock C in connexion with the water-supply. In the original apparatus a Bunsen valve was fitted into the side tube; that is, the caoutchouc tube D D was stopped at the upper end with a short piece of glass rod and cut along its length near the end by a smart blow from a chisel.

The edges of the slit were thus left sharp; and on applying any outward pressure to the tube they readily adhered, making a perfectly air-tight conjunction. The valve was then pushed within the tube B, which was narrowed at the end so as to retain the caoutchouc tube perfectly air-tight. The other end of the caoutchouc tube D D was connected with the vessel to be evacuated. On allowing the water from the main to flow through the vertical tube, the caoutchouc tube commences to pulsate rapidly as it falls over the upper edge of the tube A, and periodically closes the opening. The Bunsen valve in consequence intermittently opens and shuts, and rapid suction is set up; and it is thus easy to obtain a vacuum equivalent to 0·7 metre of mercury. The working of the apparatus is obviously akin to that of the hydraulic ram; so easily and efficaciously does it exhaust, that it will doubtless take the place of the Bunsen filter-pump. It has the great advantage of portability over the older form, since it may be so constructed that it can be transported to any position in the laboratory: it obviates the necessity of a fall of upwards of 30 feet, and therefore requires no alteration in the existing arrangements of pipes and fittings; and, lastly, its cost need not exceed a few shillings.

There are a few disadvantages connected with the use of the caoutchouc valve above described. Owing to the gradual diminution of its elasticity by long-continued working, its efficacy diminishes after a time; it not only then fails to bring about rapid exhaustion, but so soon as the conjunction of its edges ceases to be perfect, it allows the water to flow back into the caoutchouc tube. To obviate these inconveniences another form of valve was devised. A hollow metal cone shaped like a funnel is soldered air-tight into the end of the side tube B (fig. 2). This cone is pierced near its apex with a number of holes, and into it is fitted a piece of unvulcanized sheet caoutchouc shaped like a filter. This is retained in its place by a small screw passing through the sheet caoutchouc and into the apex of the cone. By its elasticity the india-rubber sheet presses firmly against the sides of the cone and effectually prevents the entrance of air or water from the tube A; but the slightest pressure from within B is sufficient to disturb the adhesion, and to allow of the ready transmission of air through the holes in the cone. This valve is of a more durable and permanent character than the original form, and permits of a more rapid exhaustion. In the modified form of the instrument a manometer M is fixed to B: this allows the degree of exhaustion to be immediately ascertained from the position of the mercury along the graduated scale. The rapidity of the pulsations in the caoutchouc tube W W may be regulated by the moveable arm T, which by means of a screw can be clamped

in any desired position. The screw S serves to regulate the rapidity of the exhaustion, or, in cases of simple aspiration, the amount of air passing through the holes in the cone. S′ is a clamping arrangement, by which the vacuum within the pump can be maintained without disturbing the screw S if it should be necessary suddenly to disconnect the caoutchouc tube D from the vessel to be exhausted.

This brief account of the slight but serviceable modifications in the original instrument of M. Jogno is made with the object of introducing an exceedingly valuable piece of laboratory apparatus to a more extended notice than it has hitherto met with in this country.

XXXI. *On the Specific Heat of Carbon. By* H. F. Weber, *Assistant in the Physical Laboratory of* Geh.-R. Helmholtz.[*]

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,   Berlin, 25th Aug. 1872.

IN yesterday's Number of the 'Athenæum,' 24th August, p. 238, I find the following:—

"British Association. Section B. *Chemical Science.*

"Mr. Dewar made a communication 'On the Specific Heat of Bodies at high temperatures, with particular reference to that of Carbon,' which he showed, from the results of a very carefully conducted series of experiments, increased in a very rapid manner with the increment of temperature. His observations were made at the temperature of boiling zinc, and of the oxyhydrogen blowpipe &c."

This property of carbon, however, I had previously discovered, and laid a detailed memoir on the subject before the Physical and Chemical Society of Berlin. An abstract of that memoir was published in the *Berichte der deutschen chemischen Gesellschaft* in April of this year. The latter seems to have escaped the notice of the Chemical Section of the British Association. As from this it appears that the *Berichte* are not so widely circulated in England as we in Germany think, I request you to print the enclosed brief memoir and this letter in the next Number of your valued Journal.

<div align="right">

Your most obedient,

Dr. Friedrich Weber.

</div>

In 1819 Dulong and Petit deduced, from the investigation of

twelve metals, the law that the product of atomic weight and specific heat, the so-called atomic heat, has the same value for all elements, about 6·3. In numerous trials, carried on from 1840 to 1862, M. Regnault tested the universal validity of this law of Dulong and Petit. The result was, that for most solid elements it is approximately true; only the specific heats must be determined at temperatures sufficiently below the melting-points of the elements in question. For 32 solid elements the mean of the atomic heats obtained was 6·3, with extremes of 6·76 for sodium and 5·7 for sulphur; phosphorus and silicium have considerably smaller atomic heats, namely 5·39 and 5·04, while crytallized boron has only 2·67, and crystallized carbon even as little as 1·76. Accordingly boron and carbon stand far outside the sphere of the validity of Dulong and Petit's law.

This remarkable exceptional position of carbon induced Regnault to determine comprehensively the specific heats of its various allotropic modifications. In his second memoir on specific heat (1841) he showed that the different allotropic modifications of carbon possess quite different specific heats, and that not one of them fulfils the law of Dulong and Petit. That is to say, the following were the specific heats obtained :—for

Animal Charcoal	.	.	.	0·2608
Wood charcoal		.	.	0·2415
Coke .	.	.	.	0·2017
Gas-coal .	.	.	.	0·2036
Native graphite		.	.	0·2019
Furnace graphite	.	.	.	0·1970
Diamond	.	.	.	0·1469

A second series of experiments, made in 1862, gave the values :—

0·1987 ⎫
0·2020 ⎬ for three different pieces of Canadian graphite.
0·1911 ⎭
0·1988   for Siberian graphite.
0·2000   for pure gas-coal.

Almost simultaneously with Regnault, De la Rive and Marcet investigated the specific heat of carbon by the cooling method. They found for charcoal from oil of turpentine 0·1801, for pure sugar charcoal 0·140–0·159, and for diamond 0·119 as the values of the specific heat; but they are too great, from two causes : first, the specific heat of copper, which served for comparison, was set 4 per cent. too high ; and secondly, the values given by the cooling method, for all substances which are not good conductors of heat, are always too great. The difference between the results obtained by Regnault and by De la Rive and Marcet

thus becomes extraordinarily great—so great that it is impossible to account for it by the different methods used for the determination, by errors of observation, impurity of the substances, &c.

Kopp, in his comprehensive investigation of the specific heat of solid bodies (1865), subjected that of carbon to a new determination. By means of the mixing method somewhat modified by him, he obtained for the specific heat of

Gas-coal	0·185
Native graphite . .	0·174
Furnace graphite . .	0·165,

values from 9 to 16 per cent. less than those found by Regnault. These considerably smaller values determine M. Kopp to the assumption that carbon in all its modifications has only one and the same specific heat, that of the diamond, 0·1469, and consequently that carbon forms an indisputable exception to the law of Dulong and Petit. In the numbers found by him for gas-coal and graphite, rather greater than 0·1469, he sees the influence of condensed gases and vapours; and Regnault's still higher numbers he accounts for by the heat of moistening which ensues when the heated porous substance is immersed in the water of the calorimeter.

In order to test this latter assumption of M. Kopp, a new and careful determination of the specific heat of carbon was recently (1868) undertaken by Wüllner and Bettendorf, in the memoir entitled "Experiments on the Specific Heat of Allotropic Modifications." Their procedure was in substance that of M. Kopp; only it was carried out with greater accuracy, and the heat of moistening was excluded. The specific heats obtained were :—

for gas-coal	0·2040
„ native graphite . .	0·1955
„ furnace graphite . .	0·1961
„ diamond	0·1483

But these values almost exactly agree with those found by M. Regnault. Hence Wüllner and Bettendorf conclude that "indeed essentially different specific heats belong to the different forms of carbon; and the heat of moistening of the porous forms does not, as Kopp assumes, cause their specific heat to appear too great."

A slight error, however, has slipped into the calculation of the above values, through the following circumstance. The substance under examination (1–5 grms.) was heated in a glass with water (1–5 grms.) to about 70°, and cooled in the calorimeter to about 20°. In the calculation it was assumed that the specific heat of this water was constant and equal to 1; but the mean specific heat of water between 20° and 70° is 1·004. The effect

of this apparently small variation is that the values found for gas-coal and graphite must be diminished by $1\frac{1}{2}$ or 2 per cent.* The amount of this correction for the diamond cannot be calculated from the data given in the memoir mentioned, because they appear to be vitiated by numerous misprints; most probably, however, it is of the same order as the above; and consequently the very good accordance mentioned between the values found by MM. Wüllner and Bettendorf and by M. Regnault vanishes.

In order to afford a readier view, the results of the four series of experiments just mentioned are placed together in the following Table :—

Observers.	Wood charcoal.	Gas-coal.	Native graphite.	Furnace graphite.	Diamond.	Temperature-interval.
Regnault..........	0·2415	0·2036	0·2019	0·1970	0·1469	8° to 98°
De la Rive and Marcet ...... }	0·2009	......	......	......	0·1146	3° to 14°
Kopp	......	0·185	0·174	0·165	......	22° to 52°
Wüllner and Bettendorf... }	......	0·2006	0·1919	0·1921	0·1452	22° to 70°

The preceding Table shows that all four series of experiments agree in this—that carbon in its various allotropic modifications possesses quite different specific heats, and that none of them can fulfil the law of Dulong and Petit; but it shows also that the values found by the different observers for the same modification differ widely from one another. The divergences are so great and universal that they cannot be accounted for through the different methods of observation, nor through impurity of the substances. Since, however, in the four series of experiments the temperature-intervals were each different from the others (as the last column of the Table shows), and since for each of the above modifications the values found rise and fall in a perfectly regular manner with the upper limit of the temperature-interval, it appeared to me in the highest degree probable that the cause of the total noncoincidence of the results hitherto attained might be, that the specific heat of carbon in all its allotropic modifications varies to a considerable degree with the temperature.

A closer investigation has completely verified this conjecture. *The specific heat of carbon increases with the temperature, and more considerably than that of any other substance ; the specific heat of the diamond is tripled when the temperature is raised from* 0° *to* 200° !

* About 15 per cent. if the value 1·052, found by Jamin and Amaury (1871) for the mean specific heat of water between 20° and 70°, be taken as the basis of the calculation.

The apparatus employed for the investigation was the ice calorimeter invented by Professor Bunsen. In order to be sure that I had a pure substance, and to exclude all condensation of gas and heat of moistening, I first subjected the diamond to a thorough examination. Geh.-Rath G. Rose was so good as to lend me the two largest and purest diamonds (weighing respectively 447 and 634 milligrammes) in the mineral collection here. In a preliminary trial the two diamonds were investigated separately as to their mean specific heat between 0° and 100°. Three experiments gave, for the larger,

$$0{\cdot}1431$$
$$0{\cdot}1439$$
$$0{\cdot}1432$$

Mean . . 0·1434;

and for the smaller,

$$0{\cdot}1436$$
$$0{\cdot}1439$$
$$0{\cdot}1441$$

Mean . . 0·1439

The little difference in these two mean values permitted me, in the subsequent investigations, to employ the two diamonds together, so that I could operate with a mass of 1081 milligrammes. The diamonds were heated to $t°$, cooled in the ice calorimeter to 0°; and from the amount of heat Q given out in the calorimeter, the weight G of the substance, and the temperature-interval $t-0=t$ the mean specific heat c_{0-t} between 0° and $t°$ was calculated, according to the equation

$$Q = G \cdot t \cdot c_{0-t}.$$

For 12 different temperatures, which were almost symmetrically distributed in the interval 0° to 200°, 33 determinations were made. These gave the dependence of the mean specific heat c_{0-t} between 0° and $t°$ upon the temperature t in the following slightly parabolic form :—

$$c_{0-t} = 0{\cdot}0947 + 0{\cdot}000497\,t - 0{\cdot}00000012\,t^2. \quad . \quad . \quad (1)$$

The variability with the temperature being so great, the *mean* specific heat has only a formal signification, very rarely capable of evaluation. In such a case, from the *mean* the *real* specific heat for the temperature t must be derived—*i. e.* that quantity of heat which the unit of weight at $t°$ requires in order to raise

its temperature $1°$. This real specific heat γ_t can be derived from c_{0-t} in the following manner :—

$$t \cdot c_{0-t} = \int_0^t \gamma_t \cdot dt.$$

We obtain

$$\gamma_t = 0\cdot 0947 + 0\cdot 000994\,t - 0\cdot 00000036\,t^2. \quad . \quad . \quad (2)$$

According to this, for

$0°$	$\gamma = 0\cdot 0947$	$150°$	$\gamma = 0\cdot 2357$
$50°$	$\gamma = 0\cdot 1435$	$200°$	$\gamma = 0\cdot 2791$
$100°$	$\gamma = 0\cdot 1905$		

Want of snow prevented me from investigating in an equally comprehensive manner the remaining allotropic modifications of carbon. With the rest of my stock of snow I made only two more determinations, upon a piece of very pure native graphite of 951 milligrammes. The graphite was heated to $34°$, and gave

$$c_{0-34} = 0\cdot 1439\,;$$

heated to $100°$ it gave

$$c_{0-100} = 0\cdot 1937.$$

From these two determinations it would follow that

$$c_{0-t} = 0\cdot 1167 + 0\cdot 0008\,t, \atop \text{and} \quad \gamma_t = 0\cdot 1167 + 0\cdot 0016\,t. \Big\}$$

Although the constants of these equations may not have the utmost accuracy, still it is perfectly evident as the result of these two experiments that the specific heat of graphite also considerably increases with rising temperature. By this the numbers hitherto found for graphite, so widely diverging from one another, are brought into almost perfect accordance.

Lastly, when we consider that for wood charcoal De la Rive and Marcet found $c_{3°-14°} = 0\cdot 2009$, while Regnault obtained $c_{8°-98°} = 0\cdot 2415$, it appears hardly doubtful that also the porous form of carbon exhibits, in relation to specific heat, the same behaviour as graphite and diamond.

Now this great variability of the specific heat of carbon with the temperature makes the hitherto observed anomalous behaviour of carbon towards Dulong and Petit's law perfectly explicable. If we might assume that the validity of the relation (2) extends up to $500°$ (which, of course, is most probably not *strictly* the case), then would the specific heat of the diamond at about $525°$ have the value $0\cdot 52$, *i. e.* $\frac{6\cdot 3}{12}$, which Dulong and Petit's law requires. But with the extremely high, not yet reached melting-temperature of carbon, this behaviour might

have long ago been deduced by analogy on the ground of the experience obtained on other substances.

The result of this investigation might be regarded as a new and brilliant confirmation of the universal validity of the law of Dulong and Petit; but it might be better to look upon what we have found as a strong argument against the validity of that law; for the law loses all physical and chemical value as soon as its validity is essentially dependent on the temperature.

As the great variability found in the specific heat of carbon gives us excellent occasion to submit to closer investigation several most important questions in the mechanical theory of heat, in relation to true heat-capacity and internal work (for which the above-given material of observation does not quite suffice), I will undertake further investigations as soon as the meteorological conditions make the employment of the ice calorimeter again possible; and before all I will endeavour to ascertain the behaviour of the specific heat of the diamond at temperatures between $-100°$ and $0°$.

Berlin, Laboratory of Geh.-Rath Helmholtz,
  31st March, 1872.

XXXII. *On a precise Method of tracing the Progress and of determining the Boundary of a Wave of Conducted Heat. By* Alfred M. Mayer, *Ph.D., Professor of Physics in the Stevens Institute of Technology, Hoboken, N. J., U.S. America*[*].

IN 1870 Meusel experimented on the formation of double iodides, and on the remarkable changes of colour produced in these bodies by heat[†]. He prepared a double iodide of copper and mercury by adding, to a warm solution of mercuric iodide in potassium iodide, copper sulphate and then sulphurous acid; the resulting precipitate is of a brilliant carmine red, and (in that experimented on by me) turns to a deep chocolate-brown on heating to about $70°$ C. In order forcibly to exhibit this change of colour, Boettger moistened the iodide with weak gum-water and painted it on paper: on heating the latter the change of colour is produced; and on cooling, the iodide regains its former brilliancy.

Dr. G. F. Barker had the kindness to present me with a card so prepared; and on experimenting with it I soon perceived the valuable means it afforded of tracing the progress and of determining the boundary of a wave of conducted heat. To Dr.

Barker I am also indebted for the iodide used in the experiments I here present.

The first use I made of this substance was to track the heat conducted by bars and plates of metal[*]; and the sharpness of the boundary of the colours instigated me to test the value of this mode of experiment by applying it to a determination of the elliptical contour of the isothermal of conduction in the principal section of a quartz crystal.

Sénarmont, in his beautiful researches on this subject (*Ann. de Ch. et de Ph.* S. 3. vols. xxi. & xxii.), has carefully determined the ratio of the axes of this elliptical figure by coating a thin longitudinal section of the crystal with wax and leading through it a silver wire, by means of which heat was brought to the centre of the plate, whence it was conducted outward, and its progress and isothermal contour determined by the melting of the wax. The following are Sénarmont's experiments on a plate 27 millims. square, whose sides were parallel and perpendicular to the principal axis of the crystal :—

Exp.			Major axis.	Minor axis.	Ratios.
1.	.	.	12·50	9·75	1·28
2.	.	.	11·60	8·50	1·35
3.	.	.	10·00	7·50	1·33
4.	.	.	12·00	9·00	1·33
5.	.	.	13·75	10·00	1·37
6.	.	.	18·00	14·00	1·29
7.	.	.	15·00	12·00	1·25
8.	.	.	9·75	7·50	1·30
			Mean ratio	. . .	1·31

Sénarmont, in the above experiments, used every precaution to attain accurate results. He screened the plate from draughts of air and from radiations, kept the plate horizontal and frequently rotated it round its heated wire. After the ellipse had become constant in its form, he allowed the plate to cool, and then measured the axes of the ellipse by means of a micrometer.

In the experiments which follow I used a quartz plate 27 millims. long, 22 millims. wide, and whose thickness was 1·2 millim. Its centre of figure was pierced by a hole 1·25 millim. in diameter, through which passed the vertical conical end of a silver wire. The iodide was made into a paint with weak gum-water, and in experiments 1, 2, 3, and 4 was applied to the surface of the plate by a camel's hair pencil. In experiments 5, 6, 7, and 8 the better plan was adopted of flowing the iodide over the

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[*] The iodide is decomposed by contact with certain metals; these should be coated with a film of collodion, or electrotyped with copper, before applying the iodide.

plate and allowing the water spontaneously to evaporate. Thus we obtain a smooth evenly distributed coating, giving a sharp outline to the elliptical figure of the conducted heat. The plate was screened from radiations of the flame which heated the wire, but was not shielded from currents of air, nor was unequal radiation of the iodide specially prevented. The method of measurement was as follows : after the ellipse was well formed and of permanent dimensions, the extremities of its longer and of its shorter axes were marked by scratching through the iodide with a very slender steel point ; the plate was then removed and the lengths of the axes determined by means of dividers and a scale divided into half millimetres.

Exp.	Major axis.	Minor axis.	Ratios.
1.	12·5	9·25	1·35
2.	14·0	10·5	1·33
3.	17·75	13·5	1·31
4.	18·25	14·0	1·30
5.	12·75	9·5	1·34
6.	12·8	9·5	1·34
7.	12·8	9·5	1·34
8.	16·4	11·8	1·38
	Mean ratio	. . .	1·33

An opinion on the relative values of the two modes of experimenting can only be formed from a discussion of the two series of observations by the method of least squares. It is true that the series are not as extended as one would wish for the application of this process ; yet its results are equally fair for both. We thus have found that the

Probable error of a single determination of ratios in S.'s series is ·0267

,,	,,	,,	,,	M.'s	,,	·0170
,,	in the mean ratio		,,	S.'s	,,	·0094
,,	,,	,,	,,	M.'s	,,	·0060

From these figures we infer that Sénarmont's ratio is barely true to a hundredth, while my result can be relied on to that figure ; and if my measures had been made with a micrometer microscope, on a plate protected from unequal radiation and shielded from currents of air, I should have obtained a ratio reliable to the third decimal place.

To the higher ratio of my determination I attach no importance ; I attribute it to the peculiarity of this particular crystal ; for several measures on this plate, with a waxed surface, gave even a higher ratio than when the iodide was used. It hence appears that, to obtain the correct ratio for a given crystal, the mean ratio derived from several plates should be adopted.

The remarkable change of colour which heat produces in this iodide led me to hope that this molecular change would be accompanied by a simultaneous variation in its radiating-power. To solve this problem I made the following experiments at different temperatures, below and above the degree at which its colour changes. One side of a hot-water cube was coated with lampblack, and another side with a thick paste of iodide and gum-water; after the latter had nearly dried I sifted iodide over it, and caused this to adhere by rubbing it gently with my finger. The cube was now filled with water, in which was supported a thermometer. The water was raised to the following temperatures, and frequently agitated so as to ensure a uniform heating of the cube. The deflections produced in the galvanometer-needles by the lampblack and by the iodide were then obtained for each fixed temperature. Each deflection given below is the mean of three experiments.

Temp.	Lampblack.	Iodide.	Ratio of deflections.	Changes in colour.
60	18·75	13·75	1 : ·70	
65	22·25	17	1 : ·71	Cherry-red, and turning in spots to chocolate colour.
68	22·75	16·25	1 : ·71	Dark red, with spots of chocolate colour.
70	24·0	16·87	1 : ·70	Whole surface of a deep brown.
72	25·0	17·62	1 : ·70	Deep purplish brown.
75	26·25	18·62	1 : ·70	,, ,,
100	45·0	30·5	1 : ·67	,, ,,

The last experiment, in which the temperature of the surface was 100°, gave deflections so far exceeding those produced before, that I sought to render them comparable by removing the hot-water cube to a greater distance from the thermobattery, when I obtained the following ratio :—

Temp.	Lampblack.	Iodide.	Ratio.
100°	20°	13°·41	1 : ·67

The result was the same ratio as formerly obtained.

These experiments seem to show that the molecular change in the iodide, which causes it to act so differently in reflecting light, does not appear to have any action on its power of radiating the rays of heat of low intensity. I intend, however, to return to this investigation, provided with an apparatus giving the differential actions of two cubes and having a carefully calibrated galvanometer, and with this arrangement to test the reflecting- as well as the radiating-power of this and other iodides.

Several applications of this iodide for showing elevations of

temperature will naturally present themselves; for example, Foucault's experiment of the heating of a copper disk when rotating in the magnetic field, can be exhibited to a large audience by painting the disk with this iodide; on the disk attaining 70° C., the brilliant scarlet will change to a deep brown, to regain its former brilliant hue on cooling.

A more useful application may be made of this, or of several other more appropriate metallic compounds, by painting them on the *pillow-blocks* and other parts of machines which are liable to injurious heating from friction. Thus the machinist can, from the colours of these paints, ascertain the temperature of these sometimes inaccessible parts of moving machines.

May 20, 1872.

XXXIII. *On Electrolysis, and the Passage of Electricity through Liquids.* By G. Quincke.

[Concluded from vol. xliii. p. 525.]

§ 57.

THE distribution of free electricity in a closed circuit is known especially from Kohlrausch's experiments[*], who connected a linear Daniell's battery by a zigzag-shaped thin metal wire, and connected two different places of the circuit with the plates of a condenser. The electricity which one condensing plate assumed when the other was connected with the earth was determined by means of a Dellmann's electrometer, and was found, in accordance with theory, to be proportional to the difference of potentials (tensions) of the free electricity at the two places of the circuit. It was here quite immaterial which point of the same section of the circuit was in conducting communication with the condensers, and also how great was the resistance of the connecting wires.

Experiments were indeed made by Erman[†] on the free electricity of a column of water when traversed by an electrical current; but on the one hand the apparatus in those days were by no means so perfect as at present; and then I wanted to ascertain experimentally that distilled water, an extremely bad conductor of electricity, indicates free electricity on the passage of an electrical current through it, just as do metals or solution of sulphate of copper.

For this purpose the above-described (§ 56) glass trough, after the wires had been removed, was filled with pure distilled water. In this were immersed two rectangular platinum plates 60 millims.

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[*] Pogg. *Ann.* vol. lxxviii. p. 1 (1849).
[†] Gilbert's *Ann.* vol. viii. (1801) p. 207, and vol. x. (1802) p. 11.

in breadth, which could be moved parallel to the smaller side, and projected above the upper edge of the glass trough. Through these plates (which had been purified with hot concentrated sulphuric acid, distilled water, and ignition in a gas-flame) the electrical current was passed into and out of the distilled water, which filled the glass trough to a height of 53·5 millims. The specific conducting-power of the distilled water used was, at 15°·5 C., about one 4640 millionth of that of mercury.

In the strongest currents used in the following experiments not as much as 0·0000375 mgrm. of hydrogen was liberated in a second on the entire surface of the metal electrode; so that the polarization of the platinum electrodes might be neglected.

Thick copper wires connected the battery with two mercury-cups, which by a suitable commutator could be connected with the two brass plates of the air-condenser as used by Kohlrausch*. One condenser-plate was connected by a metal wire with the gas-pipes of the house—that is, with the earth; the distance of the two plates was then altered from 1·5 millim. to 90 millims., and now the insulated condenser-plate was connected with a Dellmann and Kohlrausch's electrometer† in order to determine the positive or negative electricity collected upon the condenser-plate. The above-mentioned mercury-cups could be connected with the two large platinum plates of the glass trough; and according as connexion was made or broken, the free electricity of the poles of the battery was determined with an open or a closed circuit.

All the wires by which connexion was established were insulated by caoutchouc, air, or glass and shellac; the connexions were made and broken by metal holders insulated from the circuit; and the battery itself, consisting of ten Grove's cells, stood upon a resin cake, the elements insulated from each other. The glass thread by which the beam of the electrometer was suspended exhibited but very slight elastic reaction; and by preliminary experiments a table was constructed for the electrometer in the manner described by Kohlrausch‡, in order at once to determine the quantity of electricity from the deflection of the arm of the electrometer.

Although I proceeded in all cases exactly as did Kohlrausch, I was surprised that I only occasionally found the same electrical tension at the two poles of the open circuit. In some cases the negative, and in some cases the positive preponderated, even when the free electricity of the poles was determined without the condenser, the electromotive force of the battery and the delicacy of the balance being sufficient for this. The perfect insulation of the

* Pogg. *Ann.* vol. lxxxviii. p. 465 (1853).
† Ibid. vol. lxxii. p. 353 (1847).   ‡ Ibid. vol. lxxii. p. 385.

elements from each other is here of very great influence; yet we cannot calculate on getting results accurate to 5 per cent. with positive or negative electricity, unless the battery be set up with freshly amalgamated zinc and platinum plates, with concentrated nitric acid and dilute sulphuric acid of 1·2 specific gravity, and the cylinders have lain for several days in just such sulphuric acid. I could not bring the accuracy of the observations nearer than 2 per cent. of the entire value, except when I took the mean of a great many experiments, while Kohlrausch attained an accuracy of 1 per cent. Kohlrausch, indeed, like other observers, met with greater differences in the tensions of the free electricity of the two poles than I have given; but he used elements which were filled with badly conducting liquids. But the liquids in my Grove's battery were the best conductors known (apart from metallic mercury); and hence it was extremely surprising, notwithstanding this, to find differences in the tension of the free electricity of the two poles (compare § 61). Of course this difference of the two poles is met with in the closed as well as in the open circuit.

The following Table gives under $+e$ and $-e$ the quantity of positive or negative electricity of the condenser-plate when the platinum plates in the glass trough were at the distance l in the first column, where l therefore represents the length of the column of water traversed by the current. Between the separate determinations with the closed circuit, determinations were made with the open circuit, in order to control the constancy of the electromotive force. The mean of all these experiments is given opposite to $l=\infty$.

Column of water 60·6 millims. in breadth, 53·5 millims. in height.

l.	$+e$.	$-e$.	Mean.
∞	11·251	$-11·125$	11·19
400	11·26	$-11·23$	11·24
200	11·30	$-11·26$	11·28
100	10·84	$-11·30$	11·07
50	11·04	$-11·30$	11·17
20	11·30	$-11·19$	11·24
10	11·33	$-11·23$	11·28

Temperature $= 14°·6$ C.

According to theory, the quantity of electricity which collects upon the condenser-plate is proportional to the difference of potentials (tensions) of the free electricity at those places of the

interpolar wire of the circuit which are in conducting communication with the condenser-plates. This difference does not vary, whether the circuit is open or is closed by a resistance which is very great as compared with that of the circuit itself. In accordance with this these experiments show the same quantities of $+e$ or $-e$ even with varying values of l.

When the circuit was closed by columns of water of smaller section I obtained the same result.

§ 58.

In another series of experiments the current of a Grove's battery of ten elements traversed, in the manner described above, a column of water 440 millims. in length, 60·4 millims, in breadth, and 46·5 millims. in height. The brass plates of the condenser at a distance of 1 millim. were connected by insulated copper wires with the poles of the open circuit, or of the circuit closed by the column of water, but ultimately with two Wollaston's electrodes, which were at a distance of 101 millims. from each other in the water which was traversed by the electrical current. The Wollaston's electrodes consisted of platinum wires 0·076 millim. in thickness, fused in glass tubes of 2 millims. diameter for a length of 35 millims. By the two circular platinum surfaces at the end of these electrodes two analogous points of two sections of the column of water were connected with the condenser, which sections were therefore at a distance of 101 millims. from each other. The quantity of electricity accumulated in one condenser-plate (while the other plate was connected with the earth) must theoretically be proportional to the difference of potentials of the free electricity in the two sections of the linear conductor traversed by the current which are connected with the condenser, and must thus be proportional to the distance of these sections. Hence it must be immaterial which point of one and the same section we connect with the condenser.

The following Table contains in the second column, under l, the distance of the two sections of the column of water connected with the condenser, under e and $-e$ the positive or negative charge of the condenser-plate. The fifth column gives the mean of these two observations, and the last (sixth) column the value of this mean calculated from the charge with the open circuit. The numbers in the first column give the order in which the experiments were made. In No. 4 each Wollaston's electrode was placed in the corner, in No. 5 in the centre of the corresponding rectangular section of liquid.

No.	*l.*	*e.*	*−e.*	Mean.	
				e observed.	*e* calculated.
1.	∞	14·82	−15·09	14·955	
2.	400 millims.	14·795	−15·335	15·065	14·955
3.	∞	14·58	−14·79	14·685	
4.	101 millims.	2·854	− 3·47	3·162	3·370
5.	101 „	2·90	− 3·25	3·075	3·370

<div align="center">Temperature = 17°·5 C.</div>

From this we see that the observed values of *e* are somewhat smaller than the calculated ones; yet the differences come within the errors of observation, which occur to almost the same extent with observations on metallic conductors. For comparison's sake I may adduce the charge of a condenser when the same Grove's battery of ten cells was closed by various coils of copper wire, the total resistance of which amounted to 8491 m. u. In the following Table *l* gives in mercury units the resistance of that part of the metallic conduction the ends of which were connected with the condenser-plates, the distance of which was 1·5 millim. The other letters have the same meaning as before.

No.	*l.*	*e.*	*−e.*	Mean.	
				e observed.	*e* calculated.
1.	∞	11·26	−10·95	11·105	
2.	8491	11·26	−10·93	11·095	11·10
3.	5987	7·81	− 7·18	7·495	7·823
4.	3496	4·55	− 4·82	4·685	4·568

In all these, as in the earlier experiments, the numbers given are the means of several determinations.

I will here observe that when the two Wollaston's electrodes freshly ignited in a gas-flame, while the constant current of a Grove's battery traversed the column of water, were connected with the ends of a very delicate reflecting galvanometer instead of the condenser, so that a branch current traversed them, an irregular yet gradual increase of this current was observed. In this experiment the original intensity of the current, in whatever part of the section the electrodes may have been placed, may be doubled and even more, while the necessarily concomitant polarization of the electrodes should properly produce a diminution in the intensity of the current. The cause of this phenomenon is to be found either in the fact that the minute platinum surfaces, in spite of the preceding ignition, are contaminated by evaporated glass, and are therefore at first not completely moistened by the water, or that glass is dissolved by the distilled water

and there is an increased degree of conductivity in the vicinity of the platinum electrodes. This change in the conducting-power of the water by the solution of the glass covering of the electrodes would then explain the imperfect agreement of the observed and calculated values of the condenser-charges in the Table at the beginning of this section; for there would thereby be produced a change in the curves of the current: the electricity would no longer flow in the direction of the normal of the section of the column of water; and so the potential of free electricity could not be constant within the same section.

§ 59.

The preceding experiments have the disadvantage that they require a condenser, the thick insulating shellac layers of which may readily become electrical without its being noticed, and thereby occasion a tendency to one of the two electricities. The electrometer which I used is not free from this objection.

Hence, in order as much as possible to avoid this source of error, I have recently made similar experiments with a Thomson's quadrant electrometer[*], which may easily be brought up to such a degree of sensitiveness that a condenser may be dispensed with, even when a Grove's battery of only a few cells is used. Its indications are also more perceptible, quicker, and less open to external accidental sources of error than those of Dellmann and Kohlrausch's electrometer.

Thomson's electrometer consists essentially of a horizontal thin aluminium plate which has a bifilar suspension by two cocoon threads, and by means of a thin platinum wire is placed in conducting communication with a constantly charged Leyden jar, the outer coating of which is in conducting communication with the earth. Fixed to and above the aluminium plate is a small silvered concave mirror with a radius of about a metre, which projects the image of a narrow petroleum flame upon a horizontal scale. The position of the image determines, in the usual manner, the deflection of the aluminium plate.

The aluminium plate is suspended within a hollow space, which has the form of a cylindrical box made of thin metal foil and separating into four quadrants insulated from each other and open inwards. Each two quadrants which belong to the same diameter are in conducting communication with each other. Both pairs of quadrants are symmetrically arranged in reference to the aluminium plate; and this does not alter its position when both pairs of quadrants are in conducting communication or have the same electrical tension.

But if one pair of quadrants is connected with one, and the

other with the other pole of a voltaic battery, the aluminium plate, charged positively, will be repelled by the positive quadrants and attracted by the negative ones ; the deflection of the image is proportional to the difference of the electrical tensions on the two pairs of quadrants. It is quite immaterial whether on one pair the electric tension is zero or not.

The instrument I used gave a deflection of about 40 mill deflection when the poles of a Grove's element were conne with the two pairs of quadrants.

When the electrical current of a Grove's element of seven cells was passed through the trough filled with distilled water in the manner described in § 57, while the platinum electrodes were connected by a thin silver wire with the pairs of quadrants, the following deflections were obtained, according to the length of liquid (given in the first column) between the electrodes, and the passage of the current in one direction or the other through the liquid.

Length of interposed column of liquid.	Deflection of electrometer.
∞	320 . . −322
433 millims.	317 . . −317
216 „	313 . . −313

When the current of a Grove's battery of seven elements was passed in the manner described in § 58 through a column of water 435 millims. in length, the following deflections were obtained, according to the distance l of the electrodes which were connected with the pairs of quadrants of the electrometer, and according to the direction in which the current traversed the galvanometer. In experiments 1 and 2 the pairs of quadrants were connected with the large platinum plates at the ends of the column of water 435 millims. in length ; in 3 and 4 with the above-described Wollaston's electrodes, each of which was in the middle or corner of the section in question. The deflection was not altered when the Wollaston's electrodes, kept at a constant distance by being cemented to a strip of glass, were moved in the liquid parallel to the direction of the voltaic current.

No.	l.	Deflection of electrometer.			
		+e.	−e.	Mean.	
				Observed.	Calculated.
1.	∞	305	−308	306·5	306·5
2.	435 millims.	306·7	−306·7	306·5	306·5
3.	100 „	69	− 69	69	70·4
4.	100 „	67·5	− 67·5	67·5	70·4

In these experiments, as well as in those of § 58, the deflection or difference of potentials was smaller the more the specific conducting-power of the liquid was increased by the solution of the glass in the water and thereby the resistance of the interposed column of liquid diminished.

§ 60.

In the previous experiments the resistance of the battery was very small as compared with the resistance of the part of the circuit whose ends were connected with the pairs of quadrants of the electrometer. In future this shall not be the case.

If nG is the electromotive force of a Grove's battery of n elements, R the resistance of the circuit and of the conducting-wires which convey the current of the intensity J to a metallic or liquid resistance W, and if U and V represent the value of the potential of free electricity at the beginning and end of a conductor of the resistance W, then

$$J = \frac{nG}{R + W} = \frac{U - V}{W}. \quad . \quad . \quad . \quad . \quad . \quad (1)$$

For a conductor of the resistance w we should have, with the same circuit and analogous notation,

$$i = \frac{nG}{R + w} = \frac{u - v}{w}, \quad . \quad . \quad . \quad . \quad . \quad (2)$$

or, from these two equations,

$$u - v = (U - V) \frac{1 + \dfrac{R}{W}}{1 + \dfrac{R}{w}}. \quad . \quad . \quad . \quad . \quad (3)$$

Calling the difference of potentials for an open circuit $U_0 - V_0$ corresponding to the resistance $W = \infty$, then

$$u - v = (U_0 - V_0) \frac{w}{R + w}. \quad . \quad . \quad . \quad . \quad (4)$$

The resistance R in the circuit may be determined in the usual manner, by observation of the current-intensities i and J on a galvanometer, from the equation

$$R + w = \frac{J}{i - J} (W - w),$$

provided W and w are known in mercury units.

To test the accuracy of the method of observation, a Grove's battery of six elements was closed by the resistances given in the

first column of the following Table and a multiplier of suitable intensity with mirror-reading. The magnet, which was a steel ring in a copper sheath with a plane silvered mirror, came to rest in a few seconds. From the ends of the resistance W, which consisted of German-silver spirals, thin silver wires passed to the pairs of quadrants of the electrometer. The values of the current-intensity i and the values of $u-v$, measured by the deflections of the electrometer, are measured in arbitrary divisions of the instrument, and are the mean of two positive and two negative deflections. The calculation was made with the aid of equations (2) and (4), under the assumption that the resistance R of the circuit and of the conducting-wires amounted to 10 mercury units.

	Multiplier. $i.$		Electrometer. $u-v.$	
$w.$	Observed.	Calculated.	Observed.	Calculated.
∞	0	0	248·08	245·7
1000 m.u.	12·70	13·14	243·45	225·6
100 ,,	113·12	120·6	218·75	206·7
50 ,,	204·37	221·1	200·87	165·4
20 ,,	442·28	442·28	170·75	

The deviations of the observed and calculated values are for measurements with the multiplier and the electrometer of about the same order, and may be explained by the difficulty of avoiding alterations in the resistances on the passage of the current. As resistance decreases in liquids with increase of temperature, but increases in metals, the influences of the heating of the two will compensate each other in the action on the multiplier; but in the action on the electrometer they will act in the same direction.

The difficulty of keeping the resistance constant is far greater in liquid conductors—for instance, solutions of cupric sulphate; for their resistance changes with the temperature to a far greater extent than that of the metals.

The electrical current of a Grove's pile of six cells was passed through a reflecting multiplier of suitable delicacy, and by means of copper electrodes through an aqueous solution of cupric sulphate which filled a glass trough 440 millims. in length and 60 millims. in breadth to a height of 40 millims. The electrodes were copper plates of the same section as the glass trough, and were connected by thin silver wires with the pairs of quadrants of the electrometer. Instead of the solution of sulphate of copper, a series of German-silver spirals of equal resistance could, by means of a rheostat, be interposed in the circuit, so that the thin silver wires led to the ends of this metallic resistance.

In both cases multiplier and quadrant electrometer indicated the same deflections, which almost agreed with the theoretically calculated values, as seen in the following Table:—

Substance.	w.	Multiplier. i.		Electrometer. $u-v$.	
		Observed.	Calculated.	Observed.	Calculated.
Metal	∞	0		246·25	
400 mil. CuSO⁴.	70 m. u.	138	138	185·5	185·5
Metal	do.	136·7	138	185	185·5
200 mil. CuSO⁴.	35 m. u.	217·9	221·1	143·2	148·4
	do.	212·7	221·1	137	148·4

$$R = 23\cdot1 \text{ m. u.}$$

Other experiments gave similar results. It would follow thence that metallic and electrolyzable resistances have the same deportment.

When, by means of Wollaston's electrodes of thin copper wire, two different sections of the solution of sulphate of copper were placed in conducting communication with the pairs of quadrants of the electrometer, while the electric current of a battery of six cells passed through the solution, the electrometer showed a constant deflection when the electrodes while at a constant distance were moved parallel to the direction of the current, and were in the corner or in the middle of a section.

§ 61.

The difference of the electrical potential, already mentioned in § 57, which the positive and negative poles of a Grove's battery show when the other pole is in connexion with the ground, is very striking. The irregularities show themselves in the same manner with Dellmann-Kohlrausch's electrometer, as well as in the Thomson's quadrant electrometer, though the latter instrument has the advantage of measuring the difference of the electric potentials at two points of a conductor; hence the potential for a given point of the conductor may be 0.

For the sake of an example I here give a series of measurements obtained with a Grove's battery of six cells, freshly put together with the greatest possible care: the porcelain vessels of the single elements stood in a deep dry glass dish; the metal contacts were soldered and varnished with shellac; the thin silver wires of the pairs of quadrants of the electrometer were joined to the platinum and zinc plates of the first and last elements respectively by means of thick cotton-covered copper

wire, thickly coated with wax, and were, besides, completely insulated from one another.

No.	No. of elements.	Deflection of electrometer.		Mean.	Diff.
		+e.	−e.		
1.	6	243·2	−256	249·6	12·8
2.	6	242·5	−258	250·2	15·5
3.	3	114·7	−132·5	123·6	17·8
4.	3	122	−123	122·5	1
5.	6	252	−247	249·5	−5

The observations follow one another in the order in which they were made. With observations 2 and 3 one of the pairs of quadrants was connected with the ground (through the gas-pipes of the house) ; with the others they were both completely insulated. We see that the mean of the positive and negative deflections, for the same electromotive force, is almost constant; and for half that force it has half the value, as the theory requires; on the other hand, the difference of the positive and negative deflections varies, being sometimes positive and sometimes negative, independently of the electromotive force.

In another series of experiments with a new Grove's battery of from 1 to 8 cells, the difference-potentials of the interpolar wires were measured one after another. The pairs of quadrants were not in connexion with the ground.

No. of elements.	Electrometer		Diff.	Defl. corr. to 1 elem.
	+e.	−e.		
1.	44	− 43·5	−0·5	43·75
2.	87·2	− 86·3	−0·9	43·37
3.	128·8	−132·1	−0·7	44·15
4.	177	−176·9	−0·1	44·24
5.	222	−222·6	+0·6	44·46
6.	265·7	−267	1·3	44·39
7.	308·7	−311·3	2·6	44·29
8.	353·7	−356	2·3	44·31

Other experiments gave similar results.

I confess that I am not able to explain these variations of the positive and negative indications of the electrometer, which appear to change continually—since neither the assumption of an imperfect insulation of the pairs of quadrants, nor that of an electromotive force of a variable magnitude, which might have its origin in the earth, is sufficient to account for the phenomena.

§ 62.

From equation 4, § 52, the conclusion was drawn that for a given liquid a separation of partial molecules, or, what is the same thing, a chemical decomposition of the liquid, can only occur when the current has acquired a certain density. If, however, the effective electric force in the liquid conductor remain below a certain magnitude, no decomposition and no electric current can take place; but if this electric force exceed this definite magnitude, then many molecules will be suddenly decomposed simultaneously, and a current be suddenly perceived.

Clausius* also arrived at this conclusion from considerations which are similar in many respects to those communicated in this paper, and according to him are in complete contradiction to experiment, since " by means of alternate decomposition and recomposition even the smallest force produces a conducting current, and the intensity of the current is, according to Ohm's law, proportional to the force."

Clausius tries to reconcile theory and experiment by assuming that there are free partial molecules in the liquid, which have been formed and continue to form themselves through the decomposition of a total molecule, even without an electric current traversing the liquid. The electrical forces then operate merely on these *free* positive and negative electric partial molecules, increase the path which is described through this molecular motion by the free partial molecules in the direction of their action (that is, in the direction or in the opposite direction of the current); and thereby the separation of the partial molecules on the electrode is brought about.

I now think, however, that it is not necessary to make this last assumption, because the above-mentioned law is not at variance with experiment.

It is of course correct that we always perceive polarization when an electrical current of a constant battery is passed through a liquid which in ordinary language is said to conduct electrolytically, and that this polarization arises from the partial molecules which are separated by the current. If, however, in the expression for the force K, which is necessary for the separation of the partial molecules, instead of the intensity of the current, we introduce the electromotive force of the battery employed, then we obtain expression (6),

$$K = \frac{n G}{l} (B\epsilon - B'\epsilon'),$$

which shows that with the same given liquid (for which the

* Pogg. *Ann.* vol. ci. p. 347 (1857).

magnitude $B\epsilon - B'\epsilon'$ is constant) the force K is perfectly independent of the section of the conductor, and increases with the magnitude nG, the electromotive force of the constant battery employed. This electromotive force, however, is taken in the experiments by no means so exceedingly small, simply to prevent the cessation of the electrical current in consequence of the polarization which is set up; and it is very possible that, for a very small value of the quotient $\dfrac{n\text{G}}{l}$, no electrochemical decomposition would be perceived. There are certainly a number of liquids which cannot be electrolyzed, which do not conduct the constant current of a liquid battery. There are, in my opinion, such liquids with respect to which the magnitude $B\epsilon - B'\epsilon'$ has a very small value, and which for a short distance, and by the application of a constant battery of a sufficient number of elements, might very likely undergo chemical decomposition. The value of this magnitude $(B\epsilon - B'\epsilon') = A(C\epsilon - C'\epsilon')$ depends on the constants denoted by CC' and $\epsilon\epsilon'$.

I shall afterwards return to the value of the latter. With solid bodies, the constants C and C′ are very small, because the molecules are hardly mobile, and with solid salts no electrochemical decomposition is observed. The liquids which (though their particles are certainly mobile) do not conduct electrolytically, are insulators. The constant $B\epsilon - B'\epsilon'$ is consequently with them very small; and in them at all events only very great electromotive forces, great values of nG, can bring about a decomposition.

It follows from this consideration that constant batteries, at least with the number of elements which are usually at our disposal, will probably not afford the means of electrolyzing those liquids which at present are called insulators, but that frictional electricity or induction-currents (that is, electricity of high tension) must be applied in order to produce decomposition. When, for instance, the inner and outer coatings of a strongly charged Leyden battery are united by means of a column of such an insulating liquid, then the electromotive force is given by the difference of potentials of the free electricity on the inner and outer coatings of the Leyden battery, and we can then perceive a chemical decomposition of the liquid, particularly at the first discharge, when the difference of the potentials of the electricity is still great. Whether the section of the liquid is also of influence, or whether chemical decomposition occurs or not, cannot with our imperfect knowledge of the discharge, be decided, since Ohm's law is perhaps no longer applicable.

I have not attempted to demonstrate the matter separated in the so-called insulating liquids when the discharges of a Leyden

battery were passed. In case Faraday's law also holds for these liquids, the quantity decomposed is too small to demonstrate it; for the most delicate test (a polarization-current through the substances separated) is not possible here.

It must, however, not be forgotten that the conductivity of the liquid must change itself by jerks, through the electrochemical decomposition which takes place suddenly when the charge of the Leyden battery is gradually increased, and, further, that through such a column of liquid a strongly charged Leyden battery can only be partially discharged, *i. e.* until the difference of the potentials of the free electricity on the inner and outer coating has fallen below a certain value.

I certainly have not succeeded in proving these conclusions by experiment, because with the so-called insulating liquids, through the great density of the free electricity upon the inner coating of the Leyden battery, the particles of liquid (total molecules) themselves are electrified and are repelled by the electrode in connexion with the inner coating.

Hence arises in the liquid what Faraday* has termed a "carrying discharge," which I would translate "*mechanische Entladung.*" This mechanical conduction is also set up in powerful galvanic batteries, and has the same disturbing influence as in the experiments with the Leyden battery here described. Lapschin and Tichanowitsch observed, in pure ethylic alcohol, ether, and amylic alcohol, hardly any traces of decomposition with a Bunsen's battery of 900 elements; on the contrary, with the last two liquids an undulating motion from the carbon pole to the zinc pole took place. This electrical current by means of mechanical conduction is by no means inconsiderable; and on this account it can with difficulty be separated from the electric current conducted electrolytically, in case such occurs—the more so because both are, in a similar if not in quite the same manner, dependent on the length of the path traversed through the liquid. The quantity of electricity passed through the section of the conductor by mechanical conduction in the unit of time must, like that which is electrolytically conducted, increase with the density of the free electricity upon the inner coating of the battery and the magnitude of the section, and must decrease with the length of the column of liquid which is traversed. To this must be added that the *small intensity of the current*, together with a few other circumstances to which I shall afterwards return, render accurate measurements a matter of difficulty.

§ 63.

As the constants CC', $\epsilon\epsilon'$, and λ depend on the nature of the

* Experimental Researches, vol. i. § 1319.

whole of the partial molecules which are in contact with one another, the case may occur, that one of several salts dissolved simultaneously in the same will be decomposed, but the others or the liquid itself will not undergo decomposition.

The quantity $\dfrac{i}{q\lambda}$ in the expression 4, § 52, is the same for all molecules of the different chemical combinations contained in the column of liquid, but the quantity $(B\epsilon - B'\epsilon')$ is different. Now, if

$$(B\epsilon - B'\epsilon')\frac{i}{q\lambda} > R$$

(that is, greater than the force which, owing to chemical molecular forces, holds the partial molecules together), then a separation of the partial molecules takes place. If this is not the case, then no decomposition of the corresponding chemical combination is perceived.

This appears to be the reason why, when an electrical current passes through saline solutions, there is usually only a decomposition of the salt and not of the solvent; for the electrical forces are sufficient to separate the partial molecules of the salt from one another, but not those of the solvent.

According to expression 4, a decomposition of the water in the same aqueous saline solution must be set up when the section of the liquid is diminished.

The opinion that in the same aqueous saline solution it is sometimes the water which is decomposed and sometimes not, has been already advanced by Magnus[*], who pointed out the limit of intensity of the current by which the decomposition of a salt or of one constituent of a saline solution occurs (which he terms limit or limiting value[†]) as independent, first of the magnitude of the electrodes, secondly of the tendency to decomposition of the different constituents of the electrolytes, and thirdly of the proportion in which these are found in the solution.

This view of the matter was opposed by Hittorf[‡], who conceived the phenomenon observed by Magnus, that at the cathode, with a current of definite density in a solution of blue vitriol, no hydrogen is liberated, to be a secondary chemical effect, because hydrogen *in statu nascente* has the property of reducing copper from blue vitriol.

Now, in order to decide this question, I have performed the following experiments. A prism-shaped trough, 136·5 millims. long, 25 millims. broad, and 50 millims. high, made of glass plates fastened together with sealing-wax, was divided into two

[*] Pogg. *Ann.* vol. cii. (1857) p. 33, § 61.
[†] Ibid. p. 15, § 31.   [‡] Ibid. vol. cvi. (1859) pp. 348–357.

compartments of equal size by a thin plate of mica parallel to the smaller side of the prism. The two compartments communicated by a very small aperture of about 0·2 millim. diameter, and were filled to a height of 42·5 millims. with concentrated solution of pure sulphate of copper. The current of a Grove's battery of 77 cells passed into one compartment and out of the other by means of two copper plates coated on the back with sealing-wax, which were of the same size as the smaller sides of the trough, and were placed parallel to them.

Thus the electrical current flowed within two cones with rectangular base, the summits of which touched one another at the opening in the mica plate. The intensity of the current was so much the greater the nearer the section of the conical conductor formed by the solution was to the opening in the mica plate.

In the course of the experiment the opening in the mica plate increased to a section of about 3·9 square millims.; and when the copper electrodes were at a distance of 30 millims. from the mica plate, ·0611 grm. of copper were deposited in ten minutes.

On closing the circuit, gas-bubbles were given off abundantly on both sides of the mica plate, and near the opening the temperature of the liquid rose several degrees. From the increase in volume also of the air-cavities in the mica it was evident that near the aperture the temperature was high, which also very probably occasioned the enlargement of the opening. At the large copper electrodes no evolution of gas was observed.

It may be observed that this rise in the temperature of the liquid, when it is unequally distributed, causes a difference of specific gravity in the two chambers and by that means a current through the opening of the mica plate from the other chamber to the one in which the greater heating has taken place. Of course this current of liquid then carries along with it the small gas-bubbles.

In order to determine the nature of the liberated bubbles of gas, they were collected in inverted glass hemispheres of about 20 millims. diameter, in the top of which a platinized platinum wire was fused. When the glass vessels containing the gas-bubbles were placed near one another in a large vessel containing dilute sulphuric acid, and the platinum wires connected by means of a delicate multiplier, the current did not correspond to the current of a Grove's gas battery, as would have been the case had the separated gas consisted of oxygen and hydrogen. Moreover when an electric spark was passed through a mixture of the two gases obtained as has been described, no explosion took place. This, together with the fact that the gas-generation gradually becomes weaker and weaker, plainly shows that the gases liberated consisted of atmospheric air which, previously absorbed

by the solution of sulphate of copper, was given off in conse-
quence of the increased temperature caused by the current.

The following experiment confirms this explanation. A highly
concentrated solution of commercial sulphate of copper was
boiled for a long time in order to free it from the absorbed air.
It was poured whilst boiling into a large beaker and cooled as
quickly as possible in a freezing-mixture of snow and common
salt. When the solution was cooled to a temperature of 20° C.,
the glass trough was filled with it to a height of 42·5 millims.,
the two electrodes were placed on each side of the mica plate at
a distance of 30 millims., and the current of a Grove's battery
of 77 cells passed through it.

At first a few gas-bubbles were generated; but these pro-
ceeded from the air in the hollow spaces of the mica plate, and
soon completely ceased.

The liquid near the opening became very warm. At the cop-
per plate which served as cathode no evolution of gas was to be
seen; but it appeared abundantly when, instead of the broad
copper plate, a copper wire was dipped in the solution, although
the section of the wire was very much greater than the opening
in the mica plate.

If the density of the current (or the quantity $\dfrac{i}{q\lambda}$ in expres-
sion 4, § 52) had occasioned the decomposition of the water,
then in two places in the liquid changes must have appeared,
perhaps hydrated oxide of copper and sulphuric acid—that is, in
the places where the section q was so small that the force k suf-
ficed for the decomposition of the water. But no change in the
interior of the liquid was perceptible.

In order to be certain that I had not overlooked any sub-
stances liberated by the electrical current in the interior of the
liquid, I examined a concentrated solution of sulphate of copper
(specific gravity = 1·2) in an apparatus similar to that just de-
scribed, but of larger dimensions, with the apparatus contrived
by Toepler[*].

A trough made of very perfect glass plates, 200 millims. in
length and 60 in breadth, was filled to the height of 45 millims.
with a solution of blue vitriol, and divided into two chambers by
a mica plate which had a hole in its centre ·22 millim. in dia-
meter. Two large copper plates 60 millims. in breadth con-
ducted the current of a Grove's battery of twenty cells into and
out of these compartments. The distance of the copper plates
from the mica plate varied from 30 to 100 millimetres.

The light of an argand lamp, passing through an opening of

[*] *Beobachtungen nach einer neuen optischen Methode.* Bonn, 1864. 8vo.
Phil. Mag. vol. xxxiii. p. 75.

·25 of a millim. diameter in a horizontal direction, fell on a plane-convex crown-glass lens, of which the focal distance was 1 metre and the diameter 100 millims. The rays traversed the solution of sulphate of copper in the glass trough almost at right angles to the direction of the electrical current. The glass trough was placed close to the crown-glass lens, and projected, at a distance of about 3 metres behind the lens, a real image of the small illuminated aperture. This image, which was from 1 to 2 millims. in diameter, was so screened by a diaphragm with a straight vertical edge standing in front of the ocular of an astronomical telescope placed at the opening of the mica plate in the solution of sulphate of copper, that only a small portion of light could reach the telescope. On closing the battery, cloud-like veils were seen from the opening of the mica plate in the solution of sulphate of copper passing out on both sides of the mica plate, similar to those observed by Toepler to be produced by induction-sparks in air. The phenomenon was exactly the same with a different direction of the electrical current in the solution, and was manifestly due to the heating of the liquid in the narrow opening by the electrical current. Although the telescope commanded the whole space between the copper electrodes, apart from the phenomenon described nothing unusual could be perceived within the liquid.

When the aperture in the mica plate was enlarged, then the quantity of gas liberated here, if it had arisen from the decomposition of water, must have increased with the size of the aperture, since the intensity of the current increased in the whole circuit so long as the density of the current sufficed for the decomposition of water in the solution of sulphate of copper. This latter condition was fulfilled, as the experiment with the copper wire as cathode showed; and yet the evolution of gas diminished with the enlarging of the aperture, just because the heating was smaller.

According to this the whole phenomenon does not depend on a selective property of the electrical current, but on a purely secondary chemical action.

Whether the separated hydrogen results from decomposition of the water, or from a trace of sulphuric acid which is still present in the solution, might be very difficult to decide. On account of the exceedingly small specific conductivity of the water, we should incline to the latter view; it will, however, be difficult to give a stricter proof.

When in the same glass trough with the mica plate and a small aperture in the latter the current of the same Grove's battery of 77 cells was passed through a solution of pure common salt or sulphate of potash in water mixed with some tincture of

litmus, then I noticed on the electrodes, but not in the interior of the liquid or in the opening of the mica plate, a change of the chemical condition—though, of course, numberless bubbles of oxygen and hydrogen gas were developed on the anode and cathode.

In vessels in which water was poured over a concentrated solution of the above-mentioned salt, on passing the current a change was observed at the boundary of the water and the solution of salt, as Faraday* also had already found a separation of hydrate of magnesia at the boundary of water and a solution of sulphate of magnesia when the current was passed.

§ 64.

The equations in § 55 contain so many unknown magnitudes that it is for the most part extremely difficult to determine the latter more accurately. Only in a few particularly simple cases can any thing be said about the magnitudes C and ϵ.

The simplest case is that of a salt fused by heat. We may then neglect the summation and the index r, and equations (21), (23), (24), and (16) give

$$\epsilon = -\epsilon',$$
$$\frac{m}{a} = i,$$
$$\lambda = p \cdot \epsilon (C + C'),$$
$$\lambda = p \cdot \frac{\epsilon^2}{\epsilon} (C + C'),$$

or

$$\epsilon = 2.$$

This relation must hold as long and as accurately as Faraday's law holds, therefore for every liquefied salt which is decomposed by means of the electrical current at any given temperature. The quantity of electricity 2 is measured by the same unit as the current-intensity i; and that quantity of electricity has been put $= 2$ which decomposes one equivalent of an electrolyte through which it passes. Hence we have this proposition:—

If any salt in a liquefied condition is decomposed by the electrical current, then there is a quantity of free electricity $+2$ on the complex atom which forms the one partial molecule, and a quantity of free electricity -2 upon the complex atom which forms the other partial molecule.

The quantity of electricity $+2$ or -2 is just as great as that positive or negative quantity of electricity which must flow through the liquid conductor in one or the other direction in order to decompose this one equivalent of salt.

* Experimental Researches, vol. i. p. 495.

This is the more remarkable because it holds for a great series of combinations in which the liberated partial molecules, according to modern theoretical views, may be univalent or plurivalent. On account of the difficulty of separating the direct action of the electrical current from the secondary of the liberated substances, we shall not for the present be able to decide with certainty whether the electrical current only splits up the total molecules, as appears to be mostly the case, into partial molecules of an equal number of units of affinity (compare § 55).

The aforesaid relations will not hold when a salt is dissolved in water, alcohol, or the like.

§ 65.

Equation (24) § 55 gives the dependence of the specific conductivity of a liquid on the constants C_r, ϵ_r, p_r, &c. Since as good as nothing is known of these quantities, only the known facts regarding the conductivity of the electrolytes can in general be compared with that expression.

The conductivity increases with the concentration of the solution of salt, with p, and with the heating, because through the latter the constants C, C' become greater and the particles more easily displaceable. If the concentration increases too much, so that by this means C, C' decrease more quickly than p increases, then the maximum of conductivity for a definite concentration will be reached.

All this is in accordance with experiment. Thus Hankel[*] found that saturated viscous solutions of salt at once conducted far better when by heating they were deprived of this viscosity. Wiedemann[†] afterwards showed that for diluted watery solutions of $CuSO^4$, $Cu\,2NO^3$, $Ag\,NO^3$, H^2SO^4, KHO, NH^4NO^3, the specific resistance is directly proportional to the viscosity of the liquid, and inversely to the quantity of salt it contains.

Beetz[‡] has examined very accurately the conductivity of solution of sulphate of zinc, and has found that in all concentrations the conductivity increases as the temperature rises—most of all near 0°, then more slowly. With a definite concentration the maximum of conductivity occurs. Schmidt[§] found it was the same for common salt, Paalzow[||] and Kohlrausch and Nippoldt[¶] for sulphuric acid dissolved in water.

[*] Pogg. *Ann.* vol. lxix. p. 263 (1846).
[†] Ibid. vol. xcix. p. 231 (1856). Wiedemann, *Galvanismus,* vol. i. p. 425.
[‡] Pogg. *Ann.* vol. cxvii. p. 17 (1862).
[§] Ibid. vol. cvii. p. 556 (1859).
[||] *Berl. Monatsber.* 1868, p. 490.
[¶] Pogg. *Ann.* vol. cxvixxii. p. 385 (1869).

Simultaneously with the temperature and concentration, the quantities ϵ and ϵ' also undergo changes; for Wild[*] discovered that the electromotive forces between solutions of salts change with the concentration of the solutions.

With the nature of the solvent all constants of the expression (24) must also change; and it cannot be surprising that when the same salt is dissolved in water it conducts better than when dissolved in absolute alcohol, and this solution, again, better than a solution in amylic alcohol, as Hittorf[†] found—or when Connel[‡] asserts that etherial solutions do not conduct at all, and alcoholic solutions conduct very badly, whilst an aqueous solution of the same salt is easily decomposed by an electrical current.

It even appears as if those constants changed with the state of aggregation; for Hittorf[§] observed that a dilute aqueous solution of hydrochloric acid is easily decomposed by an electrical current, whilst gaseous hydrochloric acid with the same number p of equivalents in the unit of volume does not conduct the electricity of a hydrobattery.

It may be observed that this matter would be far more difficult to understand if we were to assume that the constituent molecules of a chemical combination continually separate themselves and again unite, and in electrochemical decomposition there is merely an augmentation of the path of the free partial molecules (compare § 62).

As through the presence of another salt in a solution the constants C, C', ϵ, ϵ' must be changed, the formulæ show that the conductivity of a mixture of salts will not stand in a simple relation to the conductivity of the pure solution of a salt, as is in fact shown by the experiments of Paalzow[‖].

§ 66.

Equations (17) and (27) of § 55 show that the quantities of separated substances on the electrodes, as well as the proportion of these substances, when several chemical combinations contained in the same liquid are decomposed simultaneously, must be independent of the density of the current; and this conclusion is confirmed by experiment.

The theory leads to this without presupposing Faraday's law; and all experiments which prove the latter prove at the same time the above conclusion, since Faraday's law holds independently of the density of the current. It is moreover this law

* Pogg. *Ann.* vol. ciii. p. 384 (1858).
† Ibid. vol. cvi. pp. 550, 552, & 553 (1859).
‡ Phil. Mag. vol. xvii. p. 353 (1841).
§ Pogg. *Ann.* vol. cvi. p. 585 (1859).
‖ *Berl. Monatsb.* 1868, p. 490.

which has been proved by numerous measurements by Faraday[*], Buff[†], Soret[‡], Hittorf[§], and others. And I have convinced myself of this by experiments with watery solutions of blue vitriol, which at different degrees of concentration and very different densities of the current confirmed the law to within 0·8 per cent. of exactness.

With respect to the proportion of the quantities of salt decomposed simultaneously, Hittorf[||] found that, with an aqueous solution of iodide of potassium and chloride of potassium, the proportion of the quantities of chlorine and iodine was not altered when an electrical current was passed through the liquid for a long time and the density of the current varied in the ratio of 1 : 3 or 1 : 4.

The current passed through a liquid which contained an equal number of equivalents of iodide of potassium and chloride of potassium, also liberated an equal number of equivalents of iodine and chlorine; in an aqueous solution of 3·157 equivalents of KCl and 1 equiv. of KI, 3·157 equivs. of Cl to 1 equiv. of I were liberated by the same current. The partial conductivities of KCl and KI appear from this to stand, in the same solution, in the ratio of the number of equivalents $\left(\dfrac{p_1}{p_2}\right)$ which are contained in the unit of volume of the solution; hence

$$C_1 e_1 - C'_1 \epsilon'_1 = C_2 \epsilon_2 - C'_2 \epsilon'_2.$$

As, however, all the constants in this equation may change with the nature of all the substances contained in the liquid, it does not at once follow from this, as Hittorf conjectured, that the resistance of a solution of KI is equal to the resistance of a solution of KCl when both contain the same number of equivalents in the same volume, although other experiments made this relation probable[¶]. But the above expressions accord with his view that the resistance of the electrolytes stands in no kind of relation to the chemical affinity of their ions as soon as a decomposition of the corresponding chemical combination takes place.

Also, in conformity with the above expressions, from a mix-

* Experimental Researches, 387 &c.
† Liebig's *Annalen*, vol. lxxxv. p. 1 (1853); vol. xcvi. p. 269 (1855); vol. cx. p. 284 (1855).
‡ *Ann. de Chim.* (3) vol. xlii. p. 257 (1854).
§ Pogg. *Ann.* vol. lxxxix. p. 177 (1853); vol. xcviii. p. 1 (1856); vol. ciii. p. 1 (1858); vol. cvi. pp. 337 & 518 (1859).
|| Pogg. *Ann.* vol. ciii. p. 50 (1858).
¶ It is surprising, for instance, that under similar conditions $H^2 SO^4$, HNO^3, HCl dissolved in water show nearly the same conductivity, about one 13,000th part of that of mercury (at 20°); likewise $MgSO^4$, $ZnSO^4$, $CuSO^4$ about one 20,000th part, and so on.

ture of dilute sulphuric and hydrochloric acid, with varying density of the current, Buff* obtained at the anode a mixture of chlorine and oxygen of almost the same composition.

§ 67.

It follows from equation (15), § 55, that in the unit of time different numbers of partial molecules are conducted through the same section to the anode and to the cathode, numbers proportional to the intensity of the current and to a constant; this latter has different values for different liquids, but for the same liquid is independent of the section of the thread of liquid, or, consequently, independent of the density of the current.

This is in agreement with experiment.

$\dfrac{M}{a}$ and $\dfrac{M'}{a'}$ denote the number of partial molecules which are conducted to the cathode and anode in the unit of time respectively. The proportion of the two magnitudes can be found approximately by chemical analysis of the solution in the vicinity of the electrodes before and after the current has passed, provided the electrolyzed salt (or acid hydrate, which in this case as well as heretofore may be included in the expression salt) be dissolved in water or some other liquid.

At the same time no account is taken of the solvent, although upon every partial molecule of this solvent a certain quantity of free electricity will be accumulated. If the force K given by expression (4) (§ 52) is great enough to separate the partial molecules of this solvent, then simultaneously with the dissolved salt this solvent will also be decomposed; and thus it would be only in the case of both partial molecules moving with equal and opposite velocity, of

$$C\epsilon \text{ being } = -C'\epsilon',$$

that, as regards the molecules of the solvent, the concentration of the liquid would remain the same in every part. If K is not great enough, and the sum of the quantity of electricity accumulated on the molecules of the solvent not equal to 0, then a movement of the total molecules of the liquid to the anode or cathode takes place, and consequently also a simultaneous change in the difference of concentration produced by the magnitudes $\dfrac{M}{a}$ and $\dfrac{M'}{a'}$.

As soon as in different places of the liquid the specific gravities are different, gravity and diffusion will likewise change the concentration of the different portions.

In an extensive series of solutions of salts and hydrated acids

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* Liebig's *Annalen,* vol. cv. p. 156 (1858).

in water and alcohol, Hittorf[*], Wiedemann[†], and Weiske[‡] determined the composition of the liquid near the electrodes, both before and after the passage of the electrical current; and at the same time, assuming Faraday's electrolytical law, they determined the intensity of the current by means of the quantity of silver or copper liberated in a voltameter by the same current.

With very few exceptions, to which I shall presently return, the passage of the electrical current produced a decrease of the concentration of the solution at both electrodes. Therefore the relation 11 a (§ 54) holds,

$$M < m, \quad M' < m';$$

or ϵ end ϵ' have opposite signs, and the two partial molecules move in the electrolyzed thread of liquid in opposite directions.

In Wiedemann's 'Galvanism,' vol. i. pp. 356–361 and p. 383, there is a tabulated collection of the results obtained by the above-named observers. If the number of equivalents of partial molecules which accumulate on the cathode for 1 equivalent of the metal separated in the voltameter be denoted by n, on the anode by n', then the experiments, in agreement with the theoretical considerations of § 54, give

$$n + n' = 1,$$

and n and n' independent of the density of the current.

The following Table contains a collection of the results obtained by the different observers: n and n' have the meaning just indicated, but P denotes the weight of water which was contained in the solution for 1 part by weight of salt.

Salt.	Hittorf.		Wiedemann.	
	P.	n'.	P.	n'.
H^2SO^4 \{	5·415	0·174	13·06	0·177
	23·36	0·177	21·12	0·163
$AgNO^3$ \{	14·50	0·525	19·73	0·494
	49·44	0·526	52·58	0·507
	P.	n.	P.	n.
$CuSO^4$ \{	18·08	0·325	26·37	0·333
	39·67	0·355	32·07	0·323
	76·88	0·349	44·19	0·363

* Pogg. *Ann.* vol. lxxxix. pp. 177–211 (1853); vol. xcviii. pp. 1–33 (1856); vol. ciii. pp. 1–56 (1858); vol. cvi. pp. 337–411, pp. 513–586 (1859).

† Ibid. vol. xcix. pp. 177–215 (1856).

‡ Ibid. vol. ciii. pp. 466–486 (1858).

Salt.	Hittorf.		Weiske.	
	P.	n'.	P.	n.
K Cl	3·837	0·502	8·24	0·517
	5·485	0·500	23·15	0·514
	97·87	0·508	52·73	0·518
NaCl	3·472	0·648	9·85	0·686
	20·71	0·634	34·01	0·685
	104·76	0·628	117·7	0·680
CaCl²	3·95	0·727	10·21	0·689
	20·92	0·683	26·40	0·688
	138·26	0·673	80·83	0·692
BaCl²	8·39	0·642	11·81	0·531
	79·6	0·616	22·79	0·528
	126·5	0·614	185·6	0·533

Notwithstanding the very different circumstances under which the various observers performed their experiments with different apparatus, yet they give nearly the same value of n. Owing to the disturbing influences already mentioned, we certainly cannot expect greater concordance, and may put

$$n = \frac{n}{1-n} = \frac{\dfrac{M}{a}}{\dfrac{M'}{a'}} = \frac{C\epsilon}{C'\epsilon'}.$$

In most cases n differs from $\frac{1}{2}$; and then the partial molecules move with different velocities to the electrodes, as follows immediately from equations (7) and (17).

§ 68.

With a few solutions of salts, and certainly the concentrated solutions of I² Cd, Cl² Cd, Cl² Zn, I² Zn in water or diluted solutions of the same salts in absolute alcohol, and I² Cd in methylated alcohol, Hittorf[*] observed an increase of the concentration at the anode; and this led me to conjecture that here we had the case given in expression (11b) (§ 54), that both ϵ and ϵ' would have the negative sign.

Hittorf assumes that the partial molecules are always propelled in opposite directions by the electrical current, and endeavours to reconcile this with the phenomenon discussed in the foregoing sections, by assuming that the concentrated solutions of the salts in question contain double salts, which consist of two or more equivalents of the corresponding salt, for example I² Cd, and are acted upon by the electrical current like double salts formed of two different metals. As the electrical current resolves aqueous

[*] Pogg. *Ann.* vol. cvi. pp. 542–555 (1859).

solutions of double chloride of mercury and potassium into $(Cl^2 Hg + Cl^2) 2 K$, or of double iodide of cadmium and potassium into $(I^2 Cd + I^2) 2 K$ (where the brackets include the complex molecule liberated at the anode), so iodide of cadmium is supposed in a similar manner to be resolved into $(I^2 Cd + I^2) Cd$, and that the interpretation that cadmium and iodine are both driven to the anode by the electrical current is obviously to be rejected*.

From the theoretical views here developed this appears to me not at all necessary, and the hypothesis that ϵ and ϵ' are both negative not more speculative than the assumption of such double salts. The only question is whether the conclusions derived from that hypothesis are in contradiction to actual facts.

If I take for an example of those solutions of haloid salts an alcoholic solution of iodide of cadmium, when the partial molecules of the salt are negatively electrical the total molecules of the alcohol, according to equation (21) (§ 55), are positively electrical, and will therefore be conducted to the cathode and thus accelerate the increase of concentration of the solution of iodide of cadmium, so far as it arises from the independent wandering of the molecules of iodine and of cadmium to the anode. The same thing occurs when the alcohol itself is decomposed; its partial molecules are separated, whether they are electrified similarly or oppositely.

From this view of the electrolysis of such a solution of a salt doubts might arise whether the same laws hold for it as for that greater class of electrolytes in which ϵ and ϵ' have different signs. But even then the experiments known to me would contain nothing opposed to the possibility of this assumption and the theory previously developed (§§ 52 to 55).

Faraday's law holds as well for those haloid salts as for the other class of electrolytes, as Hittorf's numerous and complete researches testify; he always found that the same galvanic current liberated equivalent quantities of metal in the silver voltameter and in the electrolyzed solution of the corresponding haloid salt.

A second question is whether during electrolysis free electricity does not appear on the electrodes. I must here remark that even for the other, greater class of electrolytes this question is not yet so completely decided as its importance requires.

Experiments (§§ 57 to 61) by Kohlrausch and myself have been made on the equal tension of free electricity on the poles of an open battery and of one closed by means of electrolytes. The differences between the two poles are by no means sufficiently explained; and the thought has often occurred to me

whether the electrolytical action of the battery itself might not occasion these differences. Against such an assumption, however, is the fact that sometimes the positive and sometimes the negative pole preponderates.

The following experiments were for the purpose of ascertaining whether, when an alcoholic solution of iodide of cadmium is electrolyzed, greater differences would appear than in the earlier experiments.

To this end a glass trough 106 millims. long and 27·5 millims. broad was filled to a height of 23 millims. with a solution which contained 1 part by weight of crystallized iodide of cadmium to 0·967 of absolute alcohol, and had a specific gravity of 1·432. The electrical current of a Grove's battery of 17 elements, which stood upon a cake of resin and was filled as described in § 57, was passed into and out of the liquid by means of amalgamated plates of cadmium which could be shifted parallel to the smaller sides of the trough. The free electricity of the poles of the battery was determined in the manner above described (§ 57), by means of a Kohlrausch-Dellmann's electrometer, but without a condenser, one pole being led to the earth, the other connected with the electrometer. When the battery was open or was closed by a column of liquid of the length l, the quantity of accumulated electricity was determined, $+e$ or $-e$ according as the positive or negative pole of the battery was connected with the electrometer. In the following Table the measurements are placed together. The notation is the same as in § 57. The first column contains the time which elapsed between fixing the battery and making the corresponding observation.

Time.	Electrometer.				
	l.	$+e$.	$-e$.	Mean.	
				Observed.	Calculated.
3 h.	∞	5·237	−4·993	5·115	
,,	160 millims.	4·957	−5·010	4·983	4·862
6 h.	∞	5·39	−5·32	5·355	
,,	160 millims.	5·04	−5·07	5·055	5·091
11 h.	∞	5·29	−5·17	5·230	
,,	160 millims.	4·91	−4·85	4·880	4·972
,,	∞	5·27	−5·24	5·255	

$R = 50$ mercury units.

Both poles had communicated nearly the same quantity of electricity to the electrometer, whether the battery was open or closed.

A similar series of measurements were made with Thomson's quadrant electrometer.

The current of a Grove's battery of 7 elements flowed through an alcoholic solution of iodide of cadmium in the glass trough described above. The amalgamated cadmium electrodes were connected with the pairs of quadrants of the electrometer by means of thin silver wires. With different distances of the electrodes the following deflections were observed :—

Distance of the electrodes. w.	Deflections of the electrometer.			
	$u-v$.		Mean.	
	$+e$.	$-e$.	Observed.	Calculated.
∞	304	−304	304	304
155 millims.	285·5	−287·2	286·3	287·8
77 ,,	278	−278	278	273·6
51 ,,	265 7	−266·2	265·9	260·5

R = 50 m. u., w (155 millims.) = 900 m. u.

The pairs of quadrants of the electrometer were further connected with two cadmium wires cemented to a strip of glass at the distance of 34·2 millims. from one another. The wires, except their extreme points, were coated with paraffin. When the cadmium plates, which conducted the current in and out, were 155 millims. from one another, then the electrometer showed the following deflections :—

Wollaston's *Electrodes.*

On the surface of the liquid . . 64 8
In the middle of the liquid . . 62·25
Calculated . . . . 63·19

The difference between the observed and the calculated deflection is explained by the accidental differences of the concentration of the alcoholic solution, just as the variations of the deflections of the electrometer when Wollaston's electrodes were moved parallel to each other.

A trough 320 millims. long, 3·6 millims. broad, 7·5 millims. high was made of glass-plate fitted together with paraffin, and filled with an alcoholic solution of iodide of cadmium. The current of a Grove's battery of 6 elements was passed through a reflecting multiplier of suitable sensitiveness and through the alcoholic solution of iodide of cadmium by means of amalgamated cadmium plates, which were connected with the pairs of quadrants of the electrometer by thin silver wires. The resistance of the entire liquid was about 2000 mercury units.

The following Table gives the intensities of the current (measured by the multiplier) and the deflections of the electrometer when the given length of liquid l is interposed between the electrodes. In observations 5, 6, and 7, 79 mercury units were interposed, besides the necessary conducting-wires, between the multiplier and the battery :—

No.	l.	Multiplier.		Mean.	Electrometer.		Mean.
		i.			$u-v$.		
1.	∞				252	−247	249·5
2.	320 millims.	107·2	105·7	106·4	248	−246	247
3.	160 ,,	210·8	209·7	210·2	249	−246	247·5
4.	90 ,,	377	376·9	377	249	−246	247·5
5.	320 ,,	102·5	101·8	102·1	248·5	−245	246·7
6.	160 ,,	211·9	211·8	211·8	248·7	−244	246·3
7.	90 ,,	372·2	370·2	371·2	246	−245	245·5

The electrometer shows almost the same deflection with different lengths of the interposed thread of liquid, as was to be expected from equation (4) (§ 60).

When Wollaston's electrodes of cadmium wire coated with paraffin were dipped into the alcoholic solution of iodide of cadmium at a distance of 35 millimetres from one another and connected with the pairs of quadrants of the electrometer, the deflection of the latter was 26·75 divisions of the scale; whilst when the cadmium plates were at the ends of the column of liquid 320 millims. long, a deflection of the electrometer of 243 divisions was observed.

The deflection of the electrometer remained the same whether Wollaston's electrodes were placed on the edge, in the middle, or on the side of the column, and almost perfectly agreed with the value 26·8 calculated theoretically.

So far as the accuracy of the method of observation reaches, all these experiments show that an alcoholic solution of iodide of cadmium behaves, as regards the distribution of free electricity when a constant current traverses the liquid, like metallic resistances of equal magnitude, or like ordinary liquid electrolytes, such as aqueous solution of sulphate of copper.

§ 69.

It is a further question whether the current in the solution of iodide of cadmium and in the metallic part of the conduction had the same intensity.

To determine this, a straight glass tube 20 millims. in diameter was filled with the alcoholic solution of iodide of cadmium

and closed at both ends with corks. Two amalgamated circular cadmium plates were soldered to copper wires which passed air-tight through the corks to the outside. In the axis of the tube a hollow glass thread of 0·7 millim. outer and 0·3 inner dia-meter was fixed, which was filled with mercury, and communicated with the outside by means of suitable openings in the cadmium plates and corks. By a small glass tube in one of the corks the apparatus could be filled with liquid without taking it to pieces.

The electrical current passed from one cadmium electrode, through a column of liquid 200 millims. long, to the other electrode, and then immediately back in the axis of the tube through the thread of mercury. Such a conductor could exercise no deflecting force on a magnetic needle when the intensity of the current was the same in the electrolytes and in the metal wire in the axis. The tube thus arranged was placed on a small slide, parallel to the magnetic meridian inclined to the horizon at an angle of 45°; and, together with the slide, its centre was placed as near as possible to a magnetized steel mirror, which was suspended in a thick copper sheath to a cocoon thread, and its oscillation regulated to 19″ by a correcting bar. When the electrical current of a Grove's battery of 20 elements traversed only the liquid in the glass tube, the magnetic mirror seen through a telescope was deflected about 330 divisions of the scale; when the current traversed the liquid column and the mercury thread in opposite directions simultaneously, then the deflection was from 3 to 5 divisions in the direction of the electrical current in the thread of mercury.

It may be observed that, in consequence of the great resistance of the liquid, the current was excessively weak, being about ·0000351 of an electromagnetic unit.

When instead of the alcoholic solution of iodide of cadmium the same apparatus was filled with a concentrated aqueous solution of sulphate of copper (specific gravity $= 1·166$) and the current of a Grove's element passed through it, there followed, since the resistance was now diminished nearly to one sixteenth of the resistance with the solution of iodide of cadmium, a deflection of 260 divisions. When the current traversed the solution of sulphate of copper and the thread of mercury in opposite directions simultaneously, then the current in the latter slightly preponderated and produced a deflection of 5 divisions.

This slight deflection is accounted for by the fact that the glass thread containing the mercury was not fixed exactly in the axis of the tube; and hence it follows that the intensities of the current in metals and in electrolytes are equal, within 1 per cent. at any rate. Kohlrausch with dilute sulphuric acid and aqueous

solution of sulphate of copper arrived at the same result *—it is true, by a somewhat more circuitous route (compare § 60),— and Buff†, too, by a method very similar to that described.

Berlin, July 1, 1871.

XXXIV. *Notice respecting some new Facts in the early History of Logarithmic Tables. By* J. W. L. GLAISHER, *B.A., F.R.A.S., Fellow of Trinity College, Cambridge*‡.

THE main facts with regard to the original calculation of logarithms are briefly as follows :—In 1614 Napier published his *Mirifici Logarithmorum Canonis Descriptio,* in which *Napierian* logarithms were introduced. Briggs perceived the advantages of the base 10, and in 1624 published the *Arithmetica Logarithmica,* containing the (Briggian) logarithms of the natural numbers from unity to 20,000 and from 90,000 to 100,000, to 14 places of decimals. There was thus left a gap of 70,000, which was filled in by Vlacq, who published in 1628, at Gouda, his *Arithmetica Logarithmica* (which he called a second edition of Briggs's work of the same name), containing the logarithms of all numbers from 1 to 100,000, to 10 places. This Table is that from which every Table of logarithms that has subsequently appeared has been copied. It contains, of course, many errors, which have gradually been discovered and corrected in the course of the 244 years that have elapsed ; but no fresh calculation has ever been made and published §, so that the results of the work performed by Briggs and Vlacq are those that still appear in our Tables. The first logarithmic trigonometrical canon was published by Gunter in 1620 ; it contains logarithmic sines and tangents for every minute to seven decimals.

Vlacq's work of 1628 was not the first Table of Briggian logarithms that was published on the Continent ; for in 1625 appeared, at Paris, Wingate's *Arithmétique Logarithmique,* giving seven-figure logarithms of numbers to 1000, and logarithmic sines and tangents from Gunter. Of the next book that appeared on the Continent, I extract the following account from De Morgan's well-known catalogue of Tables in the 'English Cyclopædia' (1861) :—

"1626. 'Tables des Logarithmes pour les nombres d'un à 10000 composés par Henry Brigge. A Goude. Par Pierre Ram-

maseyn.' The negligence of a bookbinder enables us to clear
up some confusion in rather a singular manner.   Sherwin states
that he examined his Table by one of Vlacq's, in large octavo,
printed at Gouda in 1626, of which Table we find no other men-
tion.   The Table before us corresponds in every respect except
that there is no author's name; but no one except Vlacq can be
mentioned who was in the least likely to have printed logarithms
at *Gouda* in or about 1626.   At one time we thought that *this*
Table was the original of the long series of small Tables called
after Vlacq; but this was a mistake (see 1625 * Gellibrand), and
the mistake was partly due to the following circumstances.   This
Table (Gouda, 1626), having the title, when not cut away, above
described, and which we have also seen with a Dutch title and
preface, is the Table which is always bound up at the end of the
'Sciographia, or art of Shadowes . . . . by T.† W[ells], Esq.'
London, 1635, large octavo. . . . .   That the book was intended
to have these logarithms bound at the end is evident from every
page of it.   Now the fact stands as follows:—A sufficient num-
ber of copies of the logarithms having been procured from
abroad, the binder was directed to cancel the title-page of the
logarithms, and to append them to the work.   Accordingly most
copies have no title to the logarithms, which look quite like part
of the work.   But in some copies the binder has not cancelled
as required: we have obtained two (since our first article was
written); and there is another in the library of the Royal Society.
But in all three copies the title of the logarithms is cut halfway
up with knife or scissors as a direction to the binder to cancel it.
One of our copies has the Dutch title-page to the Table, 'Hen-
rici Briggii Tafel van Logarithmi voor de Ghetallen van een tot
10000.   Ter Goude . . . . 1626.'   And the work (though the
same impression as before) has a different title-page and date,
namely, 'The Compleat Art of Dyalling . . . , by T. Wells of
Deptford, Esq.' London, 1637."

   While preparing the Report of the British-Association Com-
mittee on mathematical Tables, a copy of the original Dutch
edition of the above Table came to light in the library of the
Royal Observatory at Greenwich; and I was not a little surprised
to find that it bore the name of *Decker* on the title-page, Vlacq
being only incidentally referred to in the preface.   The full title
of this work, the copy of which (now before me) must be almost
unique, is " Nieuwe Telkonst, inhoudende de Logarithmi voor de
Ghetallen beginnende van 1 tot 10000, ghemaeckt van Henrico
Briggio Professor van de Geometrie tot Oexfort. mitsgaders de

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      † It should be I. W[ells]: the initials on the title-page are I. W.; and
the preface is signed J. W.

Tafel van Hoeckmaten ende Raecklijnen door het ghebruyck van Logarithmi, de Wortel zijnde van 10000,0000 deelen, gemaeckt van Edmund. Guntero, Professor vande Astronomie tot Londen. Welcke ghetallen eerst ghevonden zijn van Ioanne Nepero Heer van Merchistoun : Ende 'tgebruyc daer van is met eenige Arith-metische, Geometrische ende Spherische Exempelen cortelick aenghewesen, Door Ezechiel de Decker, Rekenmeester, ende Lantmeter residerende ter Goude. Ter Goude, By Pieter Ram-maseyn, Boeck-verkooper inde corte Groenendal, int vergult ABC. 1626. Met Previlegio voor thien Iaren." The preface, which occupies two pages, is signed "Ezechiel de Decker," and dated "Ter Goude den 4 September 1626." After speaking of the editions of the *Canon Mirificus*, &c., he refers to Briggs's *Arithmetica* (1624), and with reference to it proceeds, *Welck Boeck wy voorgenomen hebben tot dienste vande Onervarene inde Latijnse sprake, ende door ghebreck van Exemplaren hier te Lande, int Nederduyts te laten uytghaen, met behulp van den Kunstlie-venden Ionghman* Adriaen Vlack, *ende van ons yets by te voeghen, het welck wy noemen* Het Tweede Deel Van de NIEVWE TELKONST (which book we have designed to publish in Dutch, with the help of the art-loving young man Adrian Vlacq, for the service of those unacquainted with the Latin language, and on account of the want of copies in this country, and to add something by ourselves, the which we call the second volume of the new Arith-metic). Decker then remarks that as the printing &c. of the larger work must necessarily occupy some time, he has published the present book in the mean while ; and after describing its con-tents, he hopes it will satisfy the reader *" tot dat wy met Godts hulpe het Groote Werck voleyndicht hebben, waer in het wonderlick Maecksel ende aenghenaem Ghebruyck inde Arithmetische Geome-trische ende Spherische Werck-stucken breeder verclaert sal wesen, als mede hoe datmen tot alle getallen, soo heele als gebrokens, Lo-garithmi sal stellen, ende ter contrarie. Geniet ondertusschen dit, ende soo het u wel behaeght, sal het toecomende beter doen. Ter Goude den 4 September* 1626 " (until we with God's help have completed the great work, wherein the wonderful making and pleasant use in Arithmetical, Geometrical, and Spherical work shall be more fully explained, and also how one shall set log-arithms to all numbers, both integral and fractional, and *vice versá*. Enjoy in the mean time this, and if it pleases you, the future shall do better).

In 1628 Vlacq's *Arithmetica* was published in Latin, without a word of reference to this previous publication by Decker ; and it is owing to this fact and the extreme scarcity of the work itself that Decker's name is so completely unknown that De Morgan unhesitatingly attributed (and with good reason) any work

printed at Gouda in 1626 to Vlacq.   In his preface to the *Arith-metica*, Vlacq says that somehow ("nescio quo fato") Briggs's work came into his hands about two years previously, and that, perceiving its value and finding it unknown to the mathemati-cians of his country, he thought it would be very useful if he republished it, reducing the number of decimals given from 14 to 10 and filling in the gap of 70,000.   He was deterred, how-ever, by the consideration that Briggs was himself taking steps to accomplish the same object; but having regard to the fact that the latter could not complete the work (to 14 places) for some time on account of the great labour of such a task and his professorial duties, and being uncertain besides whether, when it was published, there might not be some difficulty in procuring copies, which even if obtained would not be very useful unless printed in Latin*, he, finding he could spare the time, deter-mined to complete the Table so as to give the logarithms of all numbers from unity to 100,000, to ten decimal places.   The rest of the preface, which is not lengthy, is devoted to matters not of historical interest.

It will thus be seen that Vlacq makes not one word of refer-ence to Decker's *Telkonst*, printed by the same printer two years before; and as on every account some mention was required of this publication, the first work on a new subject ever published in the country, and of the promise contained in the preface to it, we are driven to the conclusion that Vlacq must have quarrelled with Decker, or for some other cause have had a set purpose to ignore his book entirely.   It will be noticed that Decker through-out speaks of the contemplated reprint as if it was to be by him-self, assisted only by Vlacq, and that he lays stress on the fact that it will be in Dutch.   The *Arithmetica* (1628), however, was printed in Latin; and from the remark in his preface, quoted above, it is to be inferred that Vlacq regarded this as the most suitable language.   Some light, however, is thrown on the mat-ter by the following circumstances.   In 1631, George Miller published at London, under the title 'Logarithmicall Arith-metike' &c. what he asserted was a reprint of Vlacq's *Arithmetica* of 1628, with an English introduction (Vlacq's preface was omitted, as also his list of errata).   The Table, however, was not reprinted at all, the copies being impressions from the same type as that from which the 1628 edition itself was printed†.   I have

* " Præterea etiam vel isto opere ad finem perducto, quod incertus eram, si ejus exemplaria hic commode acciperemus, multo minus si in alia lingua ederentur quam Latinâ, quorum utilitas non nisi in paucos redun-daret."

† The reasons for this assertion are given in the paper in the 'Monthly Notices of the Royal Astronomical Society,' cited in the text (p. 295, 2nd line).   The same title-page and introduction were prefixed by Miller both to

examined four copies of Miller's edition; and in a paper read before the Royal Astronomical Society on May 10, 1872, and printed in the 'Monthly Notices' for that date, which was written before I was aware of the existence of Decker's work, I made the following remark:—"Three out of the four copies mentioned contain, after the English introduction and before the Tables, a title-page on which is printed, " *Tafel der Logarithmi voor de Ghetallen van* 1 *af tot* 100,000, while the corresponding page in Vlacq's Table is inscribed, *Chiliades centum Logarithmorum pro Numerus* (sic) *ab Unitate ad* 100,000. This shows the Dutch origin of Miller's Table; and it is besides curious, as it seems to imply that Vlacq meditated a Dutch translation of his work, but the Tables intended for the purpose were bought and published by George Miller." This surmise is confirmed by Decker's statement. It is *possible*, but not very probable, that an edition of the *Arithmetica* with a Dutch introduction absolutely was issued. The fact that no such copy seems to have been met with by Hutton, De Morgan, Murhard, &c. is not conclusive as to the non-existence of any—as it is most likely, if there was a Dutch edition, that nearly all the copies would remain in Holland; for although now the Table is the only thing that is of interest, the authors at that time seemed to regard their own trigonometry, explanations, &c., which were prefixed to the Table, as the most important part of the work.

Decker's book is of octavo size, the pages being 7·6 in. by 4·5 in. It contains two Tables—the first giving ten-figure logarithms of the numbers from unity to 10,000, with characteristics and differences, and the second (to which is prefixed the title *Edmundi Gunteri Tafel van Hoeck-maten ende Raeck-lijnen, den Wortel zijnde van* 10000,0000 *deelen*) giving Gunter's logarithmic sines and tangents for every minute of the quadrant (semiquadrantally arranged) to 7 decimal places. There is a long introduction, containing explanations of the Tables, trigonometry, &c. It is incomplete in the Royal-Observatory copy, ending abruptly at p. 50, where several pages, including the title-page to the first Table, have been torn out. The four pieces of Latin verse by Andreas Junius and Patricius Sandæus are printed after the preface in this work, as in Napier's *Canon Mirificus*, except that the order is different.

I have compared the *Telkonst* with the copy of the *Sciographia* in the Royal Society's library, alluded to by De Morgan. The first Table has the title quoted at the beginning of the extract from De Morgan, 'Tables des Logarithmes' &c., and is identical

copies of the Table in Vlacq's *Arithmetica* (1628) and Briggs's (1624). This has created much confusion; and, besides, the title-page does not correctly describe the contents of the latter work.

with Decker's first Table. A comparison of any two corresponding pages is enough to establish this; but the best proof is afforded by the *g* of Logarithmi (which is Italic instead of Roman) at the top of the page the first number on which is 7801. The second Table is not the same as the second in the *Telkonst,* but gives logarithmic sines, tangents, *and secants**; further, it is not printed in the fine bold type which characterizes Decker's work (and the other Tables printed by Rammaseyn), as well as many of the subsequent small Tables printed at Amsterdam, 1681, Frankfort and Leipzig, 1757, &c., and known by the name of Vlacq. The reason for the change in this Table is evident, as Decker merely reprinted Gunter's Table. But in 1628 appeared Vlacq's *Arithmetica* (containing logarithmic sines &c. to every minute), and in 1633 his *Trigonometria Artificialis* (giving the same for every ten seconds), in both of which the results were carried to more decimals than by Gunter, and consequently very many (chiefly last figure) errors of his detected, so that by 1635 Gunter's calculation had already been superseded. It will be noticed that the title-page to the Tables in the *Sciographia* only applies to the first (or otherwise the date could not be 1626, as there given); and I have no doubt that the second Table has no connexion with the first, having been probably printed in England. Sherwin's words in the preface (1705) to his well-known Tables are, "The Tables of Logarithmic Sines, Tangents and Secants were examined by a Table of the said Vlacq, in large octavo, printed at Gouda, 1626." That there must be some error here is quite clear, as, by Vlacq's own statement, he only became acquainted with Briggs's *Arithmetica* in that year. It seems to me quite clear (for reasons that will be stated further on) that Sherwin did not refer to Decker's work; and the only explanation (and, I have little doubt, the true one) of his statement is that he made use of a copy of the *Sciographia* having a title-page to the Tables. Imagining, as De Morgan did afterwards, that the two Tables and the title-page had formed a separate work, and knowing "no one else in the least likely to have published logarithms at Gouda in 1626," he assigned the work to Vlacq. The reason why the two Tables could not have appeared together in 1626 is that the second one contains Briggs's or Vlacq's logarithmic sines &c., first published in 1628, while in 1626 only Gunter's† had appeared: the dif-

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* The six columns in the (semiquadrantally arranged) Table are headed *Sinus, Sinus Compl., Tang., Tangens Compl., Ar. Compl. Sinus, Ar. Com. Sin. Com.*

† There is a copy of Gunter's *Canon Triangulorum* (1620), which is extremely scarce, in the British Museum. De Morgan, who had never seen a copy, says it contains, besides the trigonometrical functions, logarithms of numbers from 1 to 1000 to 8 decimals; but the British-Museum copy

ference between the two is readily seen by comparing the first half dozen sines, three of which differ in the last figure. Sherwin would therefore have gained nothing by examining his Tables by any of the real copies of 1626, with all their inaccuracies. A sufficiently good reason against his having seen the edition with Decker's name on it is that, if so, he would not have attributed it to Vlacq. It is remarkable that De Morgan should not have noticed the above anachronism; his words need not imply that he ever saw a copy of the Tables separate from the *Sciographia* (except the one with the Dutch title and preface*). The (real) edition of 1626, with the French title-page, must either have contained logarithms of numbers alone—or if there was a logarithmic canon, it must have been Gunter's.

I have met with (in the Cambridge University Library) a separate copy in which the two Tables appear exactly as in the *Sciographia*, with the same title-page, which also has the snip halfway up. It would be easy to frame theories to explain this; but it is enough to merely notice the fact that the title-page and date only are true with regard to the first Table, and that the second *could not* have been printed before 1628. Decker's words *ende van ons yets by te voeghen* may refer to the filling up of the gap in Briggs's Table from 20,000 to 90,000; but it is not unlikely that only improvements in the introductory matter were meant. Decker's work leaves no doubt that to him must be assigned the credit of having been the first foreigner who published Briggian logarithms, an honour that has always been hitherto assigned to Vlacq, but one which the latter can well spare from his list of great services in the calculation and diffusion of logarithms.

I take this opportunity of alluding to the very meagre collection of facts that is generally supposed to constitute all that is known with regard to the life of Vlacq, and of giving a fuller account. The splendid invention of logarithms by Napier, the grand improvement made by Briggs in the introduction of the base 10, his great labours in the calculation of Tables, and the rapidity and industry with which Vlacq completed the work so well begun, form a unique episode in the history of arithmetic; and, considering the great attention paid to logarithms for years afterwards both by mathematicians and calculators,

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(which is bound up with Briggs's *Logarithmorum Chilias prima*, but seems perfect) contains no logarithms of numbers.

* This can only refer to Decker's book. If De Morgan had seen it for more than a moment it is impossible he could have failed to notice Decker's name. The fact also that the difference between the second Table and that in the *Sciographia* escaped his notice confirms the supposition that he could only have had a mere glance at it apart from the latter.

it is remarkable that so little information has been given about one who played so prominent a part in their first calculation. All the work done by Briggs and Vlacq was, as it were, work done for all time; and we save labour and time by the use of logarithms simply because we are enabled to utilize so much of the work performed by them, instead of having to do it ourselves over and over again as often as it is required*. The debt, therefore, that their successors owe, not only to the inventor, but also to the calculators of logarithms, is very large: and every fact connected with the original calculation is of great interest, having regard to the enormous number of subsequent calculations, of which it has, so to speak, formed part. The first mention of Vlacq's name is very likely that quoted from Decker in this notice; and the facts of his life that are beyond question are as follow. In 1626, as he tells us himself, he became acquainted with Briggs's *Arithmetica*. He spent the next two years in calculating the logarithms of the numbers from 20,000 to 90,000, and also the logarithmic sines, tangents, and secants (from the natural sines &c. of Rheticus) for every minute to ten decimal places, and in passing through the press his *Arithmetica* (which appeared in 1628). Vlacq then undertook at his own cost and under his own care the printing of Briggs's *Trigonometria Britannica*†: most of it was completed by 1631; but the book did not appear till 1633. In the same year also was published his *Trigonometria Artificialis*, the calculation of which, he tells us, he had completed three years before. Thus for seven years Vlacq was continually occupied with the calculation of logarithms; and how earnestly he worked is evident, not only from the enthusiastic tone of his prefaces, but from the amount he performed in the time.

The above facts are all that can be obtained relative to Vlacq's life from his mathematical works; and I had in vain examined all the biographical and mathematical dictionaries I could obtain access to, with the view of learning some particulars of his life after 1633, and had finally come to the conclusion that none were known, when I came upon a small 12mo book (5·1 in. by 3 in.) in the Cambridge University Library, printed by Vlacq, in which he, in 1654, gives an account of his own life.

The book consists of two works bound together, the first title-

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* "Universalis finis tabularum est, *ut semel pro semper computetur, quod sæpius de novo computandum foret ; et ut pro omni casu computetur, quod in futurum pro quovis casu computatum desiderabitur.*" Lambert, Introd. ad Supplementa &c. (1798).

† "Rogatus deinde a clarissimo Viro D. Briggio P.M. an Canonem . . . . quem ante multos annos construxerat, Meis impensis et curâ typis mandare vellem, libentissime voto ejus annui." Preface to the *Trigonometria Artificialis*.

pages of which are " Regii Sanguinis Clamor ad Cœlum adversus Parricidas Anglicanos. HagæComitum. Ex Typographia Adriani Vlacq. 1652 ;" and " Joannis Miltoni Defensio secunda pro Populo Anglicano : contra infamem Libellum anonymum cujus Titulus, *Regii Sanguinis Clamor ad Cœlum adversus Parricidas Anglicanos*. Accessit Alexandri Mori Ecclesiastæ, Sacrarumque litterarum Professoris Fides publica, contra calumnias Ioannis Miltoni, Scurræ. Hagæ Comitum. Ex Typographia Adriani Vlacq. 1654." There is also, at the end, a supplement to More's tract, " Hagæ Comitum, Typis Adriani Vlacq, 1655." The first work was (as Vlacq tells us in the second) written by an author whose name was unknown to him ; but it contains a dedication to Charles II. signed " A. Vlac." To the second work are prefixed twelve pages headed " Typographus pro se-ipso," and signed " A. Vlacq." In these he states that he was desirous of publishing the views of both parties, and there-fore waited with some anxiety for Milton's defence (which did not appear for two years) in order to print it, as he accord-ingly does ; but Milton has in it spoken of him in such a way that he feels bound to give an explanation. The words used by Milton with regard to Vlacq are, " Est Vlaccus unde gentium nescio, vagus quidam librariolus, veterator atque de-coctor notissimus, is Londini aliquandiu bibliopola fuit clancu-larius, qua ex urbe, post innumeras fraudes, obæratus aufugit. Eundem Parisiis fide cassum et malè agendo insignem, via tota Jacobæa cognovit : unde olim quoque profugus ne multis qui-dem parasangis audet appropinquare, nunc si cui opus est ba-latrone perditissimo atque venali, prostat Hagæcomitis Typo-graphus recoctus " (Defensio Secunda, pp. 20 & 21). In answer to this inexcusable language, Vlacq simply and in a dignified manner gives the account of his life from the age of 26. After stating that Holland was his country, he proceeds to describe his mathematical labours very much as is done above. At the con-clusion he remarks that his works were a source of loss to him, but that he does not regret it, as he knows he has done a service to mathematics ; he adds, " scio . . . . istos libros abhinc aliquot annos in magna æstimatione futuros, et me aliquam gratiam à posteritate accepturum," a prediction long since verified by his-tory. The account of his life after 1633 is rather too long to reproduce here ; so I give only the following abstract. At the time of printing his Tables he was neither a printer nor book-seller, and only became so in order to circulate his own works ; with this object he went to France and then to England. He lived ten years at London, at first very comfortably ; but after-wards, having excited the jealousy of certain other booksellers, they bribed some agents of Archbishop Laud to seize and confis-

cate his books. By the help of Dr. Johnston, Bishop of London, he was, however, enabled to prevent the seizure and procure a licence to sell the books he had in stock. The King's printers then offered to procure him his licence as of old, if he would buy from them copies of two books which they had printed at Laud's request. This he did, and obtained a licence for two years, with the hope of its being then renewed; but the civil war broke out at the end of that time, and he accordingly left London and went to Paris. There he got on very well for some time and published several books (most of them theological) of which he has given the titles, believing that he had a right to do so. His success made him enemies, and, after a prosecution, all his books were confiscated. He appealed to the Court of the Parliament, but was advised by his friends to accept a compromise, whereby he was allowed to retain his books, but agreed to leave the city and not return within a year; as it was a privilege of the Paris booksellers that foreigners should only come there once a year to sell their books. He then went to the Hague to spend the remainder of his days; and as far as his conduct there is concerned, he appeals to his friends. From this account it seems that Vlacq was born about 1600; and he was certainly alive in 1655. One is almost inclined to pardon Milton his abuse, seeing that thereby we are made acquainted with what would otherwise probably have always remained a mystery.

Montucla says that in 1628 Vlacq published, besides the *Arithmetica*, a French translation of it (*Arithmétique Logarithmétrique* &c.); and Murhard gives the title, but not as having seen the book. I always used to think there was a mistake, as I could find no place in which the work, though often spoken of, was described as having been seen by the writer. The existence of the book, however, is beyond question, as there is a copy in the British Museum. Vlacq is even more modest in the French than in the Latin edition, as the only occurrence of his name on the titlepage is in the following sentence:—"La description est traduit du Latin en François, la premiere Table augmentée, & la seconde composée par Adriaen Vlacq." The Introduction is translated into French; and the first Table has the titlepage, *Table logarithmetrique pour les nombres depuis l'Unité jusqu'à* 100000. In the Penny (and English) Cyclopædia and also in the *Biographie Universelle* (1863), accounts are given of Vlacq; but they are very meagre and inaccurate. In Rees's Cyclopædia he is erroneously described as "a Flemish mathematician of Ghent," and in the same work (under Briggs) as "of Targou, in Holland." In most cyclopædias &c. (including Hutton's 'Mathematical Dictionary' and Phillips's 'Dictionary of Biography') his name does not appear at all, though much space

is given up to Briggs. That Briggs's friends were annoyed at Vlacq's publication of 1628 is undoubted*; and it is to be regretted that the former was not first consulted. The feeling, however, does not appear (in spite of Norwood's assertion, quoted in the note) to have been shared by Briggs to any great extent, if we may judge from the fact of his having asked Vlacq to print the *Trigonometria Britannica*; and this is satisfactory, as, seeing that Briggs died before the completion of the last-named work, it is clear that, in point of fact, as matters turned out, Vlacq did not at all check the completion of Briggs's *Arithmetica*. The intention of the latter was to calculate the intervening 70,000 logarithms to 14 decimals; and as no one has since been willing to perform this work, it is possible that some years might have elapsed, but for Vlacq, before the gap was filled up even to 10 places. The French manuscript Tables only extend to 12 places (the twelfth figure being uncertain); so that even if they were published, Briggs's original scheme would not be completely carried out.

Briggs perceived the advantage of a centesimal division of the right angles, and made a step in this direction in the *Britannica*, where he divided the degrees, not into minutes, but into hundredths. In the *Artificialis* Vlacq expresses his entire approval of this change, but proceeds to add that, as those who were accustomed to the old system would not take kindly to the new, he was induced to calculate his Tables, in which the old sexagesimal division is retained (the intervals being 10″). Hutton has made the remark that, had it not been for Vlacq's publication, a partial reformation of the sexagesimal system might have taken place then. This seems very true; and on consideration it appears that the only time when the change could have been conveniently effected was when the use of the natural was replaced by that of the logarithmic trigonometrical canon. It is thus not a little curious that Vlacq, while expressing his approbation of the change introduced by Briggs, should have done the very thing

* The only objection I have seen is that made by Norwood in his *Trigonometrie*, 1631 (quoted by Hutton, p. 38 of the History prefixed to his Tables). The statement that Vlacq's work was "nothing like his [Briggs's], not worthy his name" is certainly untrue. The assertion, however, that Vlacq's Latin edition was "against Briggs's mind and liking" is more to the point. It should be added that it is not at all clear that Vlacq was in any way accessory to the publication of Miller's copies (about which Briggs certainly had some right to complain) as this may have been done by the bookseller on his own account. I may remark that of two editions of Norwood (second, 1641, and seventh, 1678) now before me, the passage in question appears only in the *latter*. It there forms part of "the Epilogue or Conclusion," which ought to be at the end of the trigonometry and before the Tables; but through a mistake of the printer the pages are out of order.

that rendered the latter's work nugatory, and has led to the permanent retention of the bad old system. Tables in which the division of the right angle is entirely centesimal have been published by Hobert and Ideler, Borda and Delambre, &c.; and such are to be found in Callet; but the system has apparently become too fixed to render a change possible or, perhaps, desirable now.

In conclusion, I may express a request that if any reader of this should come across any information of a special character with regard to Vlacq or the early history of logarithms, he will be kind enough to favour me with the reference to the place where it occurs.

Trinity College, Cambridge,
   September 15, 1872.

Postscript.—After writing the above notice, it occurred to me that if there was another copy of Decker anywhere, it would probably be in the Graves library at University College, London. This library (containing about 10,000 volumes), which was formed by the late Professor Graves, of Cheltenham, is perhaps the finest collection of old mathematical books in the country, and is most likely the best ever collected by an individual. The books are at present only partially arranged; so that, although there is a catalogue, there are no press marks. It is therefore possible to know what works there are in the library, but not where to find them. The catalogue contains two books by Ezechiel de Decker, viz. the *Nieuwe Telkonst* above noticed, and another work of the same date (1626). I was kindly permitted to search for these works, and, with the assistance of the librarian, had the good fortune to find the latter. Its title is "Eerste Deel vande Nieuwe Telkonst, inhoudende verscheyde manieren van Rekenen eerst ghevonden van Ioanne Nepero Heer van Merchistoun, ende uyt het Latijn overgheset door Adrianum Vlack . . . . door Ezechiel de Decker . . . . Noch is hier achter byghevoeght de Thiende van Symon Stevin van Brugghe. Ter Goude, By Pieter Rammaseyn . . . . 1626;" and it forms a quarto volume of perhaps 450 pages. The contents are :—a complete translation of Napier's *Rabdologia* of 1617, which occupies pp. 1–148; then *Ezechiel de Decker van Coopmans Rekeninghen,* which extends to page 308, and is followed by more than a hundred pages of tables of wages, money, interest, &c.; and, at the end, a translation of Stevinus's celebrated tract *La Disme* (27 pages). The preface is dated *Wt Goude den* 4 *September int Yaer* 1626, the same day as that on which the preface to the octavo work was written. It will be noticed that Vlacq's name occurs on the titlepage; and there

are several references to him in the preface : but this communication is already so long that I refrain from making quotations ; it is enough to mention that Decker states that he was unacquainted with Latin, and that Vlacq, who was then applying himself with great zeal to geometry*, made the translations for him. There does not appear to be any mention of the octavo work in the preface ; but I have not examined the book with sufficient care to be quite certain on this point. At first I thought it possible that this was the publication to which Decker referred as the *Groote Werck* in the octavo work, and not the (improved) reprint of Briggs's *Arithmetica* ; and the probability of this seemed increased by the fact that, in a printed extract from a bookseller's catalogue gummed on to the title-page, the book was described as *Eerste (en Tweede) Deel &c.* A careful reexamination of the preface to the octavo work, however, has convinced me that the words cannot possibly have any other meaning than that here attributed to them ; so that the quarto work was the *Eerste Deel*, and Briggs's *Arithmetica*, translated into Dutch, was intended to have been the *Tweede Deel*, the octavo work being merely a makeshift till the appearance of the latter. The inaccuracy in the bookseller's catalogue is accounted for by the fact that Napier divided his *Rabdologia* into two books, so that the Dutch headings run *Ioannis Neperi Eerste Boeck* and *Ioannis Neperi Tweede Boeck*, and the compiler mistook the latter for the *Tweede Deel*. I did not succeed in finding the Graves copy of the octavo work ; but this is of very little consequence, as its interest would be almost wholly bibliographical. To show not only the great scarcity of Decker's works, but also the total oblivion into which he and they have fallen, I may mention that his name does not appear at all in the catalogues of the libraries of the British Museum, the Royal Society, the University of Cambridge, or several smaller libraries that I have examined ; nor is any mention of him to be found in the most complete works of mathematical bibliography, including Lalande, Rogg, Poggendorff, Ebert, Ersch, Murhard, Heilbronner, and Kästner, the last three of which are especially valuable for notices of arithmetical works about the period of the invention of logarithms. Kästner speaks of having heard of a translation of the *Rabdologia* by Ursinus ; but he knew nothing of Decker. In conclusion I will add that Poggendorff assigns to Vlacq a book, " Ephemerides Motuum cœlestium ab a. 1633 ad a. 1636, 4to, Goudæ, 1632," to which I have seen no other reference. All authorities agree in stating that he published at Gouda in 1636 a small table of logarithms, "Tabulæ sinuum, &c."

September 23, 1872.

* " Die dem doenmael met grooten yver inde meetkonst oeffende."

XXXV. *On the Definition of Intensity in the Theories of Light and Sound. By* Robert Moon, *M.A., Honorary Fellow of Queen's College, Cambridge*[*].

IT is clear that the loudness of a note must depend on the amount of motion which it produces in the nerves of the ear. But a note may be prolonged indefinitely, in which case the amount of motion of the nerves of the ear will be prolonged indefinitely, without producing any increase in the loudness of the note.

If a note be prolonged through a definite interval of time without undergoing any change of character, either as to intensity or otherwise, the amount of motion in the nerves of the ear produced by it will obviously be proportional to the length of the interval during which the note continues; whence it is clear that the true measure of the loudness of a sound must be the amount of motion of the nerves of the ear which it occasions, divided by the time during which it operates.

And since the only test we are ever likely to get of the amount of motion of the nerves of the ear in any given case is the amount of motion of the aërial particles through the intervention of which the nerves are stimulated, it follows that we must take for our test of the loudness of a sound the ratio of the aggregate amount of motion of the particles producing it to the time of its continuance.

In estimating the aggregate amount of motion above spoken of, it is to be observed that it is not the algebraical sum of the expressions for the motion during each semiexcursion of the molecules, but the entire space travelled over by the particles in the given interval, without reference to the direction in which such motion takes place, that is to be taken into account—since it is obvious that the effect of the aërial particles in stimulating the nerves of the ear does not depend on the fact of the motion of the latter taking place in any one direction rather than in its opposite.

Hence, denoting by $2a$ the amplitude of excursion of the particles, and by n the number of excursions to and fro in a given time t, if we represent by a^2 the effect on the nerves of the ear of a single complete vibration of the particles, the aggregate effect on the nerves of the ear during the time t will be represented by na^2; and the loudness of the sound will be represented by $\frac{na^2}{t}$; that is, since $\frac{t}{n}$ denotes the time of vibration, *the intensity of a sound varies as the square of the amplitude directly, and as the time of vibration inversely*[†].

* Communicated by the Author.
† Not wishing to encumber the argument of this paper with a separate

The definition of intensity in the theory of light must of course be precisely similar.

It may be useful to compare the definition at which we have thus arrived with that ordinarily given in approved text-books on the theory of undulations. Of these I shall give four examples.

"As in the doctrine of sound, the frequency of the aërial pulses, or the number of excursions to and fro from its point of rest made by each molecule of the air, determines the pitch or note, so, in the theory of light the frequency of the pulses, or number of impulses made on our nerves in a given time by the æthereal molecules next in contact with them, determines the *colour* of the light; and . . . . as the absolute extent of the motion to and fro of the particles of air determine the *loudness* of the sound, so the amplitude, or extent of the excursions of the æthereal molecules from their points of rest, determine the brightness or intensity of the light."—*Encyc. Met.* art. "Light," No. 563 (1830).

"In the aërial pulses the amplitude of the vibration determines the *loudness* of the sound; and the frequency of the pulses, or the time of vibration, determines its *note*. In like manner, the amplitude of the *æthereal* vibrations determines the *intensity of the light*; and their frequency, or the period of vibration, determines its *colour*."—Lloyd's 'Wave Theory,' p. 14 (1857).

"When in our final results we have found the expression

$$c \cdot \sin \left\{ \frac{2\pi}{\lambda} vt + \mathrm{C} - \frac{2\pi}{\lambda} x \right\}$$

for the displacement of the particles touching a screen or touching the eye, we shall assume the intensity of the light to be represented by c^2. We shall suppose that the colour of light depends on the value of λ."—Airy, 'On the Undulatory Theory of Optics,' p. 20 (1866).

"At a certain point of its excursion, the velocity of the particle is a maximum. *The intensity of the sound is proportional to the square of this maximum velocity.*"—Tyndall, 'On Sound,' p. 11 (1867).

The contrast of the definition of intensity thus enunciated with that which I have propounded is sufficiently striking. That the latter is correct, I apprehend, there can be no manner of doubt.

A Cambridge mathematician, whose views are apt to exercise

controversy, I content myself with recording here my conviction that the effect of a single complete vibration on the nerves of the ear is properly represented by the simple power, and not by the square of the amplitude—and consequently that *the intensity of light should be defined as varying with the amplitude directly and with the time of vibration inversely.*

considerable influence, has indeed expressed to me his surprise that I could suppose the doctrine of the intensity increasing with the amplitude to be maintained independently of the restriction, *cæteris paribus*. He further states that he knows no one who maintains the doctrine I impugn, and that he is in the habit in his lectures of pointing out formally and explicitly that it is only in comparing sounds of the same pitch, or lights of the same refrangibility, that it is asserted that greater intensity and greater amplitude of vibration go together.

I cannot help thinking that if the writers whom I have cited had been as well aware as my correspondent of the limited application of the received definition of intensity in the theories of light and sound, they would have been as careful as he appears to have been in formally and explicitly indicating the fact.

To me it appears to be simply inconceivable that Sir John Herschel and Dr. Lloyd should have contrasted note and colour as depending on frequency of impulse, with loudness and brightness as depending on amplitude of excursion, if they had been familiar with the fact that loudness and brightness depend just as much on frequency of impulse as on amplitude of excursion —or that Mr. Airy should have assumed the intensity of the light represented by the above formula to be denoted by c^2, and the colour to depend upon λ, all the while knowing that the intensity depends upon λ just as much as does the colour—or that Professor Tyndall should have spoken of the intensity of sound being proportional to the maximum velocity, keeping silent as to the time of vibration, having the fact present to his mind that the intensity depends on the time of vibration just as much as on the maximum velocity[*].

But whatever may be the degree of illumination prevailing at Cambridge in regard to this subject, none such has extended to Heidelberg, as appears conclusively from the following passage:—

"Mécaniquement, l'intensité des vibrations, *pour des sons de différentes hauteurs,* est proportionnelle à la force vive, c'est à dire au carré de la plus grande vitesse des molecules vibrantes. Mais l'oreille a une sensibilité différente pour les sons de différentes hauteurs, en sorte qu'on ne peut arriver ainsi au véritable rapport (pour les différentes hauteurs) entre l'intensité et la sensation."—*Théorie Physiologique de la Musique,* par H. Helmholz. Traduit de l' Allemand par M. G. Guéroult. Paris, 1868, p. 15, n. [†].

But while expressing his consciousness of the defective

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[*] If definitions after the fashion my correspondent would attribute to the writers referred to are tolerated in the "exact sciences," it is clear that they must speedily lose all claim to the appellation.

[†] The passage of which the above is the translation is retained intact in the German edition of 1870 (see p. 20).

character of the received definition of intensity in the theories of light and sound, my correspondent preserves silence as to the mode in which the defect is to be remedied. I conclude, therefore, that I have been the first to point out the true definition of intensity in these theories.

That a principle so simple, so important, lying at the very threshold of the subject, should have hitherto been suffered to pass without recognition, is only one of abounding proofs of the crude and imperfect manner in which the theory of undulations has been treated, and of the necessity which exists for its entire revision, as well with the view of eradicating from it the errors with which it has been allowed to be mixed up, as of placing it upon a firm and intelligible scientific basis.

6 New Square, Lincoln's Inn,
    June 12, 1872.

XXXVI. On the Chemical Efficiency of Sunlight. By JAMES DEWAR, Esq.*

OF all the processes proposed to measure varying luminous intensity by means of chemical effects, not one has yet been expressed in strictly dynamical measure. This is owing to the very small amount of energy to be measured necessitating very peculiar processes for its recognition. The chemical actions generally induced by light are of the "Trigger" or "Relay" description—that is, bear no necessary relation to the power evolved by the transformation. There is one natural action of light, however, of a very different kind, continuously at work in the decomposition of carbonic acid by plants, necessitating a large absorption of energy, and thus enabling us to ascertain the proportion of the radiant power retained, through the chemical syntheses effected.

So far as I am aware, the following passage, extracted from Helmholtz's Lectures "On the Conservation of Energy," delivered at the Royal Institution in 1864, and published in 'Medical Times and Gazette,' contains the first estimate of the chemical efficiency of sunlight. "Now, we have seen already, that by the life of plants great stores of energy are collected in the form of combustible matter, and that they are collected under the influence of solar light. I have shown you in the last lecture that some parts of solar light—the so-called chemical rays, the blue and the violet which produce chemical action —are completely absorbed and taken away by the green leaves of plants; and we must suppose that these chemical rays afford

that amount of energy which is necessary to decompose again
the carbonic acid and water into their elements, to separate the
oxygen, to give it back to the atmosphere, and to collect the
carbon and hydrogen of the water and carbonic acid in the
body of the plant itself. It is not yet possible to show that
there exists an accurate equivalent proportion between the power
or energy of the solar rays which are absorbed by the green
leaves of plants, and the energy which is stored up in the form
of chemical force in the interior of the plants. We are not yet
able to make so accurate a measurement of both these stores of
energy as to be able to show that there is an equivalent pro-
portion. We can only show that the amount of energy which
the rays of the sun bring to the rank is completely sufficient to
produce such an effect as this chemical effect going on in the
plant. I will give you some figures in reference to this. It is
found in a piece of cultivated land producing corn or trees; one
may reckon per year and per square foot of land 0·036 lb. of
carbon to be produced by vegetation. This is the amount of
carbon which during one year, on the surface of a square foot
in our latitude, can be produced under the influence of solar
rays. This quantity, when used as fuel and burnt to produce
carbonic acid, gives so much heat that 291 lbs. of water could
be heated 1° C. Now we know the whole quantity of solar
light which comes down to one square foot of terrestrial surface
during one second, or one minute, or one year. The whole
amount which comes down during a year to one square foot is
sufficient to raise the temperature of 430,000 lbs. of water 1° C.
The amount of heat which can be produced by fuel growing
upon one square foot during one year is, as you see from these
figures, a very small fraction of the whole amount of solar heat
which can be produced by the solar rays. It is only the 1477th
part of the whole energy of solar light. It is impossible to de-
termine the quantity of solar heat so accurately that we could
detect the loss of so small a fraction as is absorbed by plants
and converted into other forms of energy. Therefore, at pre-
sent, we can only show that the amount of solar heat is suffici-
ent to produce the effects of vegetable life, but we cannot yet
prove that this is a complete equivalent ratio." This estimate
is, strictly speaking, the mean agricultural efficiency of a given
area of land, cultivated as forest; and considering that active
growth only takes place during five months in the year, we may
safely adopt $\frac{1}{600}$ of the total energy of sunlight as a fair value
of the conserved power, on a given area of the earth's surface
in this latitude during the course of the summer. As chloro-
phyl in one or other of its forms is the substance through
which light becomes absorbed and chemical decomposition en-

sues, it would be interesting to acquire some idea of the storage of power effected by a given area of leaf-surface during the course of a day, and to compare this with the total available energy. Here we are dealing with strictly measurable quantities, provided we could determine the equation of chemical transformation.

Boussingault's recent observations on the amount of carbonic acid decomposed by a given area of green leaf seem to me to afford interesting data for a new determination of the efficiency of sunlight. By experiments made between the month of January and October under the most favourable circumstances in atmospheres rich in CO^2, one square decimetre of leaf was found to decompose in one hour, as a mean, 5·28 cub. centims. of CO^2, and in darkness to evolve during the same period of time 0·33 cub. centim. of CO^2. In other words, one square metre of green surface will decompose in twelve hours of the day 63·36 cub. centims. of CO^2, and produce in twelve hours of the night 3·96 cub. centims. of CO^2[*].

This quantity of carbonic acid decomposed does not represent the whole work of sunlight for the time, as water is simultaneously attacked in order to supply the hydrogen of the carbohydrates. Boussingault, in summing up the general results of his laborious researches on vegetable physiology, says, " Si l'on envisage la vie végétale dans son ensemble, on est convaincu que la feuille est la première étape des glucoses que, plus ou moins modifiés, on trouve répartis dans les diverses parties de l'organisme ; que c'est la feuille qui les élabore aux depens de l'acide carbonique et de l'eau."—*Ann. de Chemie,* tom. xiii. p. 415. The fundamental chemical reaction taking place in the leaf may therefore be represented as follows :—

(1) $CO, O + H^2O = CO, H^2 + O, O$
(2) $6(CO . H^2) = C^6 H^{12} O^6$
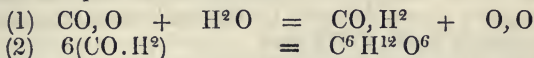
In the first equation carbonic acid and water are simultaneously attacked, with the liberation of a volume of oxygen

* The rate at which the leaf functions is dependent on the luminous intensity. The relative amounts, therefore, of carbonic acid decomposed through the action of the different coloured rays are proportional to their luminous power; and the curve of assimilation is found to follow the curve of Fraunhofer. This proves that the judgment we form of equal luminous impressions is in reality due to equal mechanical effects associated with the different coloured rays. Professor Draper, of New York, in his recent paper " On the Distribution of Heat in the Spectrum," by dividing the spectrum into two portions of equal luminous intensity, obtained identical thermal effects by absorption. This does not prove that each ray has the same total energy, but only that in all probability those at equal distances on either side of the mean wave-length in the normal light-spectrum of the sun are identical.

equal to that of the original carbonic acid, together with the formation of a substance having the composition of methylic aldehyde. The second equation represents the condensation of this aldehyde into grape-sugar. The transformation induced in (1) necessitates the absorption of a large amount of energy; and if we neglect the heat evolved in the combination of nascent CO and H^2, which can be shown to be very little, the calculated result is made a maximum; whereas the condensation of (2) being attended with an evolution of heat, diminishes considerably the amount of power required. Happily Frankland's direct determination of the thermal value of grape-sugar leaves no doubt as to the true equivalent of work done in its formation. Taking the following thermal values $CO,O = 68,000$, $H^2,O = 68,000$, $C^6 H^{12} O^6 = 642,600$, 1 cub. centim. of CO^2 decomposed as in (1) would require 6·06 gramme-units of heat, or its light-equivalent, whereas the complete change into grape-sugar of the same amount of carbonic acid requires only 4·78 gramme-units. But, we have seen before, 1 square decimetre of green leaf functions at the rate of 5·28 cub. centims. of carbonic acid assimilated per hour; therefore $5·28 \times 4·78 = 25·23$ represents the number of gramme-heat units conserved through the absorption of light in the above period of time. Pouillet estimates the mean total solar radiation per square decimetre exposed normally to the sun's rays in or near Paris per hour as 6000 gramme-units, so that $6000 \div 25·23 = \frac{1}{238}$ represents the fraction of the entire energy conserved. The estimate is by no means too great, as Boussingault has shown the leaf may function at twice the above rate for a limited time; and as both sides of the leaf are included in the measurement of the green surface in his memoir, we ought to double the fraction for a leaf exposed perpendicularly to the sun's rays, increasing the above number to the 120th part.

In connexion with equation (1), above given, as representing the action of sunlight on the leaf, it is worthy of remark that, supposing the carbonic acid and water equally efficient as absorbing agents of the vibratory energy (although each has a specific absorption for certain qualities of rays), the decomposition of the two compound molecules may take place continuously side by side, owing to the equality of the thermal equivalents of carbonic oxide and hydrogen. We already know, from the laborious researches of Tyndall, how thoroughly aqueous vapour retains thermal radiations; and Janssen has further shown the same substance has a strong absorptive action on the rays of light of low refrangibility (just those rays that are in part selected by chlorophyl), producing the well-known atmospheric lines of the solar spectrum. The presence, therefore, of varying

quantities of aqueous vapour in the atmosphere in all probability produces a considerable differerence of rate in the decomposition effected by the leaf, and may in fact end in carbonic acid and water being attacked in another ratio than that given as the fundamental equation of decomposition. Thus the same plant in different atmospheric conditions may elaborate different substances.

XXXVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 232.]

May 30, 1872.—George Biddell Airy, C.B., President, in the Chair.

THE following communication was read :—

"Examination of the Gases occluded in Meteoric Iron from Augusta Co., Virginia." By J. W. Mallet, Ph.D., M.D., Professor of Analytical and Applied Chemistry, University of Virginia.

The investigation by Graham of the gases given off by meteoric iron from Lenarto, in Hungary, when heated in a vacuum produced by a Sprengel pump, excited much interest at the time of publication*, but does not seem to have been followed up by any similar examination of other meteorites. I have made use of pieces of the iron found about three years ago in Augusta Co., Virginia, the description and analysis of which were published by me in the 'American Journal of Science' for July 1871, in order to repeat the experiment of Graham, and ascertain whether similar results to his would be obtained. A large part of the work of the extraction and analysis of the gaseous contents of this iron has been done by two of the students in my laboratory, Mr. F. P. Dunnington of Baltimore, and Mr. J. B. Adger, of South Carolina, to whom I am much indebted for their assistance.

Two preliminary experiments were made,—the first with some shavings from the cutting of the iron upon a planing-machine ; the second with a solid piece of the metal planed to smooth, clean surfaces, and quite free from any crust or scale. The shavings were subjected to the purification practised by Graham, namely, washing with a hot solution of potassic hydrate, followed by washing with distilled water and thorough drying. The solid strip of iron was not so treated, care having been taken to use no oil upon the tool employed in cutting it. Both specimens gave off gas readily when heated in the Sprengel vacuum, the amount in each case being larger in proportion to the bulk of the iron than in the experiment of Graham ; and analysis showed that the same gases were present as those found by him, with the addition of carbonic anhydride in not inconsiderable amount.

* Proc. Roy. Soc. xv. p. 502.   Phil. Mag. S. 4. vol. xxxiv. p. 239.

The final experiment was made as follows, with great care, and with all precautions which could be thought of to avoid error.

A parallelopiped of iron was cut upon a planing-machine from the largest of the three masses found (that spoken of as No. 1* in the paper above referred to), the work being done with special care, to avoid the least trace of grease being derived from the machine.

Not only was the cutting-tool itself made red-hot in the blacksmith's fire, hardened in clean water, and tempered and ground without contact with any thing greasy, but every part of the machine-bed, set-screws, and frame, from which any risk was to be feared, was carefully cleansed, and paper used to cover the whole of the iron, except where actually borne upon by the tool. The piece of iron measured about 75 millims. long, 16 millims. wide, and 12 millims. thick. It was cut from as solid a portion of the mass as could be found, and was quite bright upon the surface and free from crust, though traces of a very minute crack or fissure were barely perceptible at one end. The piece weighed 124·589 grammes ; and as the specific gravity of the iron had been found to be 7·853, the volume was 15·87 cub. centims. A new and perfectly clean porcelain tube, with sound glaze, was used, heated by a small upright fire-clay furnace with good draught, through holes in the opposite sides of which the tube was passed. The fuel was charcoal, in pieces a little larger than a walnut. The Sprengel pump had a fall-tube of about 1·34 metre long ; its connexions were made with great care, and were protected by outer casings of india-rubber tube, with the annular space between the tubes filled with glycerine. A plate of glass floating on the mercury in the funnel at top served to prevent the risk of air being carried down, as the metal was gently poured on through another and smaller funnel with narrow aperture.

A good vacuum having been obtained in the cold, lighted charcoal was placed in the furnace, and gas very soon began to come off.

It was determined to analyze separately that collected at the beginning, middle, and end of the process, in order to see whether the different constituent gases were given off at the same or at different rates. The total amount obtained was 36·33 cub. centims., reduced to 0° C., and 1 metre pressure. This was divided into three portions

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* The results of ordinary analysis were:—

Iron	88·706
Nickel	10·163
Cobalt	·396
Copper	·003
Tin	·002
Manganese	trace.
Phosphorus	·341
Sulphur	·019
Chlorine	·003
Carbon	·172
Silica	·067
	99·872

for analysis as follows :—

						h	m
Portion A....	52·02 per cent. of the whole was collected in					2	30
Portion B....	24·11	,,	,,	,,		2	20
Portion C....	23·87	,,	,,	,,		9	40
	100·00					14	30

It will be seen that the greater part came off within the first two hours and a half; but the process lasted *fourteen hours and a half*, and was not entirely over at the end even of this time. The heat had been gradually raised from dull redness to something nearly approaching whiteness at the end of the time; and when the experiment was stopped very small but still perceptible traces of gas were still coming off, though their appearance was immediately arrested whenever the temperature was allowed to fall but a little below the high point which had been reached.

The piece of iron taken out from the tube when it had become quite cold was found glazed by a thin film of fused phosphide of iron and nickel (Schreibersite), thickest on the edge which had been lowest, this phosphide having oozed out from the mass at the very high temperature used.

The tubes used to collect the gas during the first portion of the time occupied in the experiment were found slightly moistened on the inside, and the moisture, which had a distinctly acid reaction, was proved to contain hydrochloric acid, this having no doubt been derived from the chlorine existing in the iron in combination with that metal and with nickel.

Careful analysis of the gas yielded the following results by volume for the three portions separately collected: the fourth column of figures, obtained by summing up the three which precede it, gives the percentage composition of the whole of the gaseous matter extracted from the iron :—

	Portion A.	Portion B.	Portion C.	Total gas.
Hydrogen	22·12	10·52	3·19	35·83
Carbonic oxide	15·99	11·12	11·22	38·33
Carbonic anhydride .	7·85	1·02	·88	9·75
Nitrogen	6·06	1·45	8·58	16·09
	52·02	24·11	23·87	100·00

Other gases were tested for, but none could be found; no free oxygen could be detected, nor any compound of carbon and hydrogen.

From these figures it appears that hydrogen maintains about the same proportion to the other gases in A and B, but diminishes largely in C, that carbonic oxide increases in amount in B as compared with A, but remains about the same in relative amount in C, that carbonic anhydride diminishes throughout the whole continuance of the experiment, and that nitrogen falls off in B as compared with A, but largely increases again in C.

Contrasting the results with those of Graham, and noticing first the

total volume of gas obtained from the iron, it becomes necessary to
reduce this volume to the same standards of pressure and tempera-
ture employed by him.   In the paper read before the Royal Society,
as reported in its ' Proceedings,' I find no statement in regard to such
standards; but, supposing it probable that the barometer at 30
inches and thermometer at 60° F. were referred to, I have calculated
the volume of gas obtained in all from 15·87 cub. centims. of iron as
equivalent under these conditions of pressure and temperature to
50·40 cub. centims., or 3·17 times the volume of the metal.   This
is a somewhat larger quantity than that of Graham, namely 2·85
times the volume of the Lenarto iron used ; but the time of heating
was longer in the experiment now described, and the temperature
attained probably much higher.

As to the nature and relative amount of the constituent gases, the
results differ very noticeably from those of Graham, as is evident
when the figures of the two analyses are placed side by side :—

	Lenarto iron.	Augusta Co., Virginia iron.
Hydrogen	85·68	35·83
Carbonic oxide	4·46	38·33
Carbonic anhydride	——	9·75
Nitrogen	9·86	16·09
	100·00	100·00

The gases obtained in the experiment now in question agree more
nearly with those of common wrought iron (clean horseshoe-nails)
as found by Graham[*], viz. in the first portion collected,—

Hydrogen	35·0
Carbonic oxide	50·3
Carbonic anhydride	7·7
Nitrogen	7·0
	100·0

and the conclusion arrived at by him, that " the predominance of
carbonic oxide in its occluded gases appears to attest the telluric
origin of iron," would deny to the Virginia specimen the right to
be classed amongst meteoric masses, with which, however, all its
other physical and chemical characteristics agree most fully.

It is to be noted that the analysis of the gases from the Lenarto
iron was not made with the whole of the gaseous matter collected :
the first portion, amounting to about 32·5 per cent. of all collected,
was used for merely qualitative examination ; the second portion,
57·6 per cent., was that fully analyzed ; while no mention is made
of the disposition of the remaining third portion of 9·9 per cent. ;
and it is stated that the iron was not fully exhausted at the end of
two hours and thirty-five minutes, for which time only the experi-
ment was continued.   In my own experiment it appears probable
that the amount of hydrogen (and with it the total volume of gas) has

been slightly diminished by its union with chlorine of metallic chlorides to form the minute quantity of hydrochloric acid observed in the faint film of moisture on the sides of the first tubes ; and probably also this moisture itself may have been caused by the partial reduction, by means of hydrogen, of carbonic anhydride to carbonic oxide. Although it might be assumed, especially in view of the strong tendency of iron to take up and "occlude" carbonic oxide, that this gas had been the original form in which the gaseous carbon compounds obtained existed in the iron, and that it had in part broken up at the temperature of the experiment into carbon (remaining united with the iron) and carbonic anhydride (which escaped as gas), yet in view of the steady decrease in the quantity of this latter gas collected as the experiment proceeded and the temperature became higher, and bearing in mind the ready decomposition it undergoes in contact with ignited iron, it seems more likely that a larger amount of carbon originally existed in the iron in this higher state of oxidation than appears from the figures of the analysis. Although the proportion of hydrogen found is so much less in the Virginia than in the Lenarto iron, it yet represents for the former about 1·14 times the volume of the iron itself, whereas common terrestrial iron occludes but about ·42–·46 of its own volume under ordinary pressure.

I am quite satisfied, from the condition of the masses of iron as they came into my hands, and especially from the character of the crust, that the metal has not been subjected to any heating in a blacksmith's fire or otherwise by human hands since it was found, as has sometimes happened to similar specimens in the endeavour to discover their nature, or to make use of them.

Whether or not this analysis be considered as furnishing presumptive evidence of the Virginia iron having come to our earth from a different atmosphere to that of which the Lenarto meteorite brought us a sample*, the result differs so far from that of our sole previously recorded determination of the kind as to make it a matter of much interest that a larger number of meteoric irons from various localities should be subjected to careful examination in the same direction, thus supplementing our knowledge of the fixed constituents of these curious bodies by a study of their gaseous contents.

June 20.—Sir James Paget, Bart., D.C.L., Vice-President, in the Chair.

The following communications were read :—

"On some Properties of Anhydrous Liquefied Ammonia." By G. Gore, F.R.S.

This investigation was made for the purpose of ascertaining the general solvent properties of the liquid, and to detect any manifest chemical reactions between it and various substances. The method employed was precisely similar to that used in the examination of liquid cyanogen (see Proc. Roy. Soc. No. 131, 1871), the tubes

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* Some of the observations of Secchi and Huggins seem to render it probable that carbon may play an important part in some regions of the universe, though the results on this head are not as full or satisfactory as those in reference to hydrogen.

being charged with anhydrous chloride of calcium previously saturated with the ammonia vapour.

Two hundred and fifty substances were submitted to contact with the liquid, and the general results in each case recorded. The only elementary substances soluble in it were the alkali-metals proper, also iodine (bromine was not tried), sulphur, and phosphorus. The more frequently soluble inorganic salts were nitrates, chlorides, bromides, and iodides; whilst oxides, fluorides, carbonates, sulphides, and sulphates were very generally insoluble. Many saline substances, especially certain chlorides, bromides, iodides, and sulphates, absorbed ammonia freely, and swelled greatly, but did not dissolve. The behaviour of the chlorides of mercury was peculiar.

Various compounds of carbon were submitted to the action of the solution of potassium in the liquefied vapour; the free potassium disappeared, but no elementary carbon was liberated.

"On the Law of Extraordinary Refraction in Iceland Spar." By G. G. Stokes, M.A. Sec. R.S.

It is now some years since I carried out, in the case of Iceland spar, the method of examination of the law of refraction which I described in my report on Double Refraction, published in the Report of the British Association for the year 1862, p. 272. A prism, approximately right-angled isosceles, was cut in such a direction as to admit of scrutiny, across the two acute angles, in directions of the wave-normal within the crystal comprising respectively inclinations of 90° and 45° to the axis. The directions of the cut faces were referred by reflection to the cleavage-planes, and thereby to the axis. The light observed was the bright D of a soda-flame.

The result obtained was, that Huygens's construction gives the true law of double refraction within the limits of errors of observation. The error, if any, could hardly exceed a unit in the *fourth* place of decimals of the index or reciprocal of the wave-velocity, the velocity in air being taken as unity. This result is sufficient *absolutely to disprove* the law resulting from the theory which makes double refraction depend on a difference of inertia in different directions.

I intend to present to the Royal Society a detailed account of the observations; but in the mean time the publication of this preliminary notice of the result obtained may possibly be useful to those engaged in the theory of double refraction.

XXXVIII. *Intelligence and Miscellaneous Articles.*

REPORT ON A MEMOIR BY MM. F. LUCAS AND A. CAZIN, ON THE DURATION OF THE ELECTRIC SPARK. BY EDM. BECQUEREL[*].

MM. F. LUCAS and A. Cazin purposed to estimate the duration of electric sparks in some determinate circumstances, especially when the dimensions of the exciting batteries are changed, and the

* The experiments were repeated before the members of the Commission (MM. Morin, Le Verrier, Fizeau, Jamin, and the Reporter, Edm. Becquerel), at the Conservatoire des Arts et Métiers.

exploding distance, the nature of the electrodes, and the resistance of the circuit traversed by the electricity are varied.

Two methods have hitherto been employed to render appreciable the duration of a spark. One, devised by Wheatstone, consists in causing the image of the spark to be reflected upon a mirror moveable about an axis parallel to the length of the spark. Although the spark of an ordinary electrical machine exhibits no duration appreciable by this means, yet, with a certain velocity of rotation, battery-discharges give images elongated in the direction of the rotation of the mirror, a proof of a sensible duration, but with diminution of luminous intensity. With the aid of this mode of experimenting, M. Feddersen has been able to study the constitution of the discharge, and even its subdivisions.

The other method, given by Arago for the purpose of getting a limit of the duration of lightning-flashes, requires the employment of a disk moveable about an axis perpendicular to its plane. This disk is divided into sectors by radiating lines with equal intervals, and appearing bright upon the darker ground of the disk. From the widening of the lines produced by the light of the sparks their duration may be estimated when the velocity of rotation of the disk is known. This process of experimentation has been followed by M. Felici, who studied by transmission the widening of the transparent lines of an opaque disk, illuminated by the discharges of a Leyden jar, when divers circumstances of their production are varied.

MM. Lucas and Cazin have employed a method which permits easier measurements, and in certain cases more precise, than the preceding, but without distinguishing whether the sparks result from one or several successive discharges. It consists in using a moveable disk, the margin of which, intended to be viewed by transparency, is interposed between the observer and the spark to be studied. This disk, formed of plates of mica, bears on its margin transparent equidistant lines as fine as possible, obtained by photographic reproduction. It is placed in front of a second disk, opaque, of the same diameter, which remains fixed, and has on its margin seven transparent lines including six divisions, the breadth of which corresponds to that of five divisions of the moveable disk, so that this second disk forms a vernier by means of which $\frac{1}{6}$ of a division of the first can be estimated. This vernier constitutes the very ingenious novelty of the method.

The electric spark studied explodes in the focus of the lens of the collimator, which sends rays, parallel to the axis of rotation of the moveable disk, upon the lines of the fixed vernier. A telescope on the other side of the disk permits the observer to examine the luminous appearances.

If the spark has an inappreciable duration, the observer will see only a single bright line or none. In the first case the spark has flashed at the moment of coincidence of a line of the moveable wheel and a line of the fixed vernier; in the second case the spark took place between two coincidences.

The probability of coincidence, which depends on the breadth of

the lines of the disks, as well as on the number of the lines on the ver-
nier, has been determined by experiment, and has been found to be
equal to 0·70; that is to say, if an instantaneous spark be produced
at any instant whatever, it will illuminate a line 70 times out of 100,
and 30 times not. The probability might be different with another
apparatus.

Let us now suppose the duration of the spark to be a little greater
than that of the passage of a line of the moving wheel over two
lines of the vernier; then, if the spark commenced at the instant of
the first coincidence, by reason of the persistence of luminous impres-
sions on the retina the bright line resulting from this first coincidence
will be visible at the same time as that from the second, and we
shall see two lines at once. If with the same duration the spark
bursts forth between two coincidences, it has ceased when the third
arrives, and only one bright line is seen, corresponding to the second.
On this supposition, therefore, we ought to see one or two coinci-
dences of bright lines at the time of appearance of the discharges.

But if the duration of the spark is a little longer than the prece-
ding, it will be comprised between two numbers easy to determine,
the difference of which is equal to the time which elapses between
two successive coincidences. Meanwhile the approximation can be
carried further; and the authors have shown that, in consideration
of the above-mentioned probability of coincidence, by estimating the
total number of visible lines resulting from the observation of a
known number of sparks, as well as the velocity of rotation of the
moveable disk, we can deduce therefrom, with an ascertained degree
of approximation, the duration of the visible spark.

When the apparatus is in operation, only a limited number of
coincidences can be seen at one time; so that when the duration of
the spark becomes greater and is such that for a velocity of rotation
of the disk more than five or six coincidences appear at once, the
velocity has to be diminished in order to keep within the limits of
that number of coincidences; and from these two quantities the
duration of the spark is determined.

It must be remarked that, by the duration of the *visible spark*,
must be understood the time which separates the moment at which
the spark commences from the instant at which, in consequence of
the diminution of its luminous intensity, it ceases to illuminate suffi-
ciently the whole of the lines of the apparatus to give the observer
a sensible image, whatever the direction of the discharge or of its
subdivisions, while the *total duration* may be greater.

The measurement of the duration of the sparks depending on the
number of coincidences seen by the observer, if the degree of illu-
mination of the lines happens to diminish much, it is to be feared
that the number of coincidences diminishes equally, in consequence
of the weakening of the light corresponding to the end of the dis-
charge. The authors assert that, in the same conditions of produc-
tion of sparks, the measure of their duration preserves the same value
when the velocity of rotation is changed. Now, in this case the
illumination of the lines diminishes in proportion as the velocity of

rotation is augmented; so that, in the conditions under which they operated, the alteration of luminous intensity did not sensibly modify the results of their observations. Still it would be desirable that the authors should be able to explain in what manner the luminous intensity intervenes when comparisons are made between sparks of unequal brightness, especially when the discharges explode between electrodes of various metals, placed at different distances, in gases at divers pressures, and that it should be possible for them, in certain cases, to operate with the same sum of light illuminating the lines of their apparatus.

Since the coincidences of the lines of the fixed and moveable disks depend on the duration of the spark up to a certain limit of luminous intensity, it must be remarked that their number might be increased in consequence of the phosphorescence of the moveable disk; but mica being one of the solids in which the phenomenon of persistence of this luminous action is the least marked, it follows that no sensible perturbation can arise from the interposition of a moving disk of that substance between a luminous focus and the eye of the observer.

The authors of the memoir have not been able, with the apparatus constructed as it is, to render appreciable the duration of a spark proceeding from an ordinary machine; but they have ascertained that the duration of the discharges from condensers varies with the surface of these, with their disposition, and in proportion to the resistance of the circuit traversed by the electricity; it changes likewise with the exploding-distance, the nature of the knobs of the exciter, and the humidity of the air. In general the duration increases with the surface of the condenser and with the distance of the knobs, and diminishes with the lengthening of the circuit. In these researches, they have given as limits of observed durations 4 millionths of a second and 86 millionths of a second, with a possible error of 1 millionth of a second.

They have been able to represent by empiric formulæ the results obtained in divers series of observations, and have arrived at this consequence—that there is a limit towards which the duration of the spark tends when one augments indefinitely the surface of the condenser and the exploding-distance and diminishes the resistance of the conducting circuit.

Briefly, MM. Lucas and Cazin have devised an ingenious method of experimenting, which they have studied with care, and which has already led them to some very interesting results in the experiments made with condensers; but it would be important to render this method equally applicable to the investigation of the duration of sparks produced by ordinary machines without the intervention of batteries. The Commission, therefore, invite MM. Lucas and Cazin to continue their researches, and have the honour to propose to you to order the insertion of their memoir in the *Recueil des Savants étrangers*.

The conclusions of this Report were adopted by the Academy.—*Comptes Rendus de l'Acad. des Sciences*, July 8, 1872, pp. 66–69.

ON A NEW GALVANIC PILE, OF ECONOMIC CONSTRUCTION.
BY M. GAIFFE.

The high price of galvanic piles and the difficulty of procuring them being often an obstacle to the applications which might be made of them, I essayed the possibility of devising an apparatus that one could make anywhere without the aid of the professional work-man, with substances of little value, widely spread in commerce, and possessing the essential quality of constancy in the effects.

The pair which, after some trials, I have adopted, resembles Cal-laud's in its form, used some years since on telegraphic lines; but its elements are different. It consists of a vessel into which dip two rods—one of lead, the other of zinc. The leaden one descends to the bottom; the zinc is one half shorter. The bottom of the vessel is coated with red oxide of lead (minium); and the exciting liquid is water containing 10 per cent. of chlorhydrate of ammonia.

The electromotive force of this pile is about one third of that of a Bunsen's pair; its internal resistance is slight, and varies little; the chloride of zinc formed does not sensibly alter the conductivity of the exciting liquid; its constancy is great; finally the expense is almost nothing when the circuit is open.—*Comptes Rendus de l'Acad. des Sciences*, July 15, 1872, p. 120.

" ACOUSTICAL EXPERIMENTS " ETC.
To the Editors of the Philosophical Magazine and Journal.

Stevens Institute of Technology,
Hoboken, New Jersey, U.S.A.,
August 15, 1872.

GENTLEMEN,

Will you have the kindness to publish the following as a substitute for the erroneous paragraph which terminates my paper on " Acoustical Experiments &c." which appeared in the Philosophical Magazine, April 1872 ?

Very respectfully,
Your obedient Servant,
ALFRED M. MAYER.

We will now examine the analogical phenomena in the case of light. Let fork No. 1, giving 256 vibrations in a second, stand for 508,730,000,000,000 vibrations a second; which will be the number of vibrations made by the ray D_1 of the spectrum, if we adopt 300,000 kilometres per second as the velocity of light. Then fork No. 3 will represent 504,750,000,000,000 vibrations per second; which latter give a wave-length ·0000048 millimetre longer than that of D_1, and belongs to a ray removed from D_1 towards the red end of the spectrum by eight times the distance which separates D_1 from D_2. We saw that fork No. 3, giving 254 vibrations a second, had to move towards the ear with a velocity of 8·734 feet to give the note produced by 256 vibrations per second emanating from a fixed fork; so if a star which only sends forth those rays which vibrate 504,750,000,000,000 times a second were moving towards the eye with a velocity of 2442 kilometres, or 1517 miles, its colour would change to that given when D_1 emanates from a stationary soda-flame.

XXXIX. *On a Method of detecting the Phases of Vibration in the Air surrounding a Sounding Body, and thereby measuring directly in the vibrating air the lengths of its Waves and exploring the form of its Wave-surface.* By ALFRED M. MAYER, *Ph.D., Member of the National Academy of Sciences, Professor of Physics in the Stevens Institute of Technology, Hoboken, N. J., United States*[*].

Preliminary Considerations.

THE curve A, B², B⁴, &c. is the well-known symbolic representation of the dynamic condition of the air, at a given instant, when traversed by simple sonorous vibrations. The portions of the curve above the axis O X represent the lengths and manner of the aërial condensations, while the flexures below
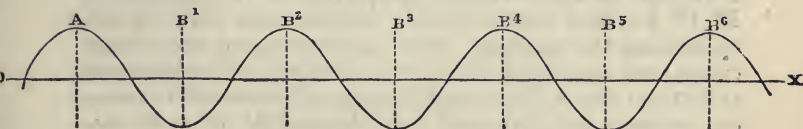
the axis represent the rarefactions; therefore similar points in the flexures above the axis, or similar points in the flexures below the axis, represent like phases of vibratory motion. Imagine these conditions of the air produced by a body vibrating at A;

then the distance A to B^1, B^1 to B^2, &c. will equal half wave-lengths, while A to B^2, B^2 to B^4, &c. will represent whole wave-lengths (English) corresponding to the note given by A. If another sonorous body, B, giving exactly the same note as A, be placed anywhere on O X, it will have vibrations communicated to it from the vibrating air, almost as though its substance was of the air itself. Now imagine this body B placed at B^2, or at B^4, or B^6, &c., then its phases of vibration will be exactly similar to those of A; but when placed at B^1, B^3, B^5, &c., its phases of vibration will be opposed to those of A. That is, at distances from A equal to any number of whole wave-lengths the body B will, at the same moment of time, swing with A, but at distances from A equal to any number of half wave-lengths the direction of its swings, at any given instant, will be opposed to A; while at intermediate positions, on the line O X, the oscillations of B will be lagging somewhat behind, or be slightly in advance of the phase of A's vibration.

From this it is evident that if, by any means, we can *see at the same time* the vibrations of A and of B, we shall (if the received conception of the nature of a vibration's propagation is correct) see their motions just as has been described above, and therefore be able to measure, *directly in the air*, a wave-length, and to determine any wave-surface enclosing a vibrating body. I have devised several processes. I will, however, here describe two only; the first, though impracticable, I speak of to render clear the general method of all; the second I give on account of its simplicity, ease of execution, and the superior accuracy of its numerical results.

Take two tuning-forks giving the same note, and having mirrors attached to their similar prongs. Place one at A, the other anywhere on the line O X. Reflect a pencil of light from each mirror of the forks on to a revolving mirror whose axis of rotation is in a plane parallel to the planes of vibration of the forks. If the fork B, which vibrates sympathetically, be placed at B^2, B^4, B^6, &c., then the two pencils reflected from the forks will, on striking the revolving mirror, be drawn into two sinuous curves, and the flexures of the two curves will run parallel with each other; that is, the curves will appear as the two rails of a sinuous railway; but if the fork B be placed at B^1, B^3, or B^5, &c., then the sinuosities of the two curves will no longer be parallel, but will be opposed to each other; that is, where the flexure of one of the curves is concave on the left, the corresponding flexure of the other curve will have its concavity on the right. If the fork B be placed at intermediate positions in reference to those above stated, we shall have neither concordance nor opposition of the flexures, but intermediate relations depending on the

fraction of half wave-length at which the sympathetic fork is placed on the line O X.

It is now readily seen that if we could place the fork B at two successive points (as B² and B⁴) on the line O X so that exact concordance of flexures should be seen at each of these points, then evidently the fork would be at two positions removed from each other by a wave-length; for at these points the air would have, at the same instants, the same phases of vibration. Thus we should have measured a wave-length. Furthermore, if by any means we could move the fork B around A so that during this motion it always preserved, in reference to A, the same relation of vibratory phase, we should determine the form of the wave-surface produced by the propagation of A's vibration.

The above is an exposition of the thoughts that have occupied my mind for several months; and they ultimately led to the following method, by which all I have mentioned can be accomplished without any difficulty—thanks to the inventive genius of M. König, to whose skilful aid so many physicists are continually indebted.

The membranes of M. König's manometric capsules furnish us with surfaces which vibrate in perfect accordance with the air which touches them, and we can lead the impulses of these membranes through gum tubes to gas-jets, placed at any desired point, where the vibrations of their flames can be compared. Thus they are far superior to tuning-forks, which require the relations of delicate adjustments to be maintained during each change of position, and therefore could only with difficulty be made to serve in the measure of a wave-length, and could not be employed at all to trace out a wave-surface, on account of the impossibility of a continuous comparison of their vibrations, which latter condition the manometric flames admirably fulfil.

The Experiments.

Let us now proceed to experiment. I placed on the acoustic bellows an open ut₃ organ-pipe, and from its ventral manometric capsule I led a tube to a gas-jet placed in front of a cubical revolving mirror. I took an ut₃ Helmholtz resonator, and adapted to its beak a gum tube, with an interior diameter of 1 centimetre, and a length of over 4 metres. This tube led to a firmly supported manometric capsule, whose flame was placed quite close to and directly in front of the organ-pipe flame; which latter had about twice the height of the resonator-flame. On sounding the organ-pipe and holding the resonator quite near it, the two flames, by a slight adjustment, were made to appear

as *one series of serrations* in the rotating mirror. Now, on gradually moving the resonator away from the pipe, I saw another series of serrations (produced by the resonator-flame) slowly evolve themselves from the first series and gradually slide over the latter, until, having removed the resonator from its first position by about 66 centimetres, or half a wave-length (German), I had the pleasure of seeing the series of moving serrations stand exactly between the first (or immoveable) series. On moving the resonator yet further from the sounding-pipe, I saw the serrations of the resonator-flame continue their onward progress until the two series again coincided; and on measuring the distance of the resonator from its first position near the pipe, I found it to be equal to the whole wave-length of ut_3. When I had removed the resonator one and a half wave-length, I again saw the serrations of the resonator-flame bisecting the spaces between the serrations given by the organ-pipe flame; and when the resonator had progressed from the pipe to a distance equal to two whole wave-lengths, I saw that the serrations of its flame had progressed to another coincidence with those of the organ pipe, and so on, until I had determined on the line of the resonator's motion *all the phases of vibration* corresponding to three whole wave-lengths. I now moved the resonator until I had again caused the serrations of its flame neatly to bisect the spaces between the serrations of the organ-pipe flame; and moving round the organ-pipe, with the resonator held at such distances from it that the bisections were steadily kept, I described in space the wave-surface of the sounding-pipe. This wave-surface I found to be approximately an ellipsoid, with its foci at the top and bottom of the pipe. Nothing could be more satisfactory; and it was charming to behold how neatly the wave-surface could be determined; for a small change in the distance of the resonator from the pipe produced a sensible shifting of the serrations.

I now substituted for the resonator an organ-pipe, in every respect similar to the one on the bellows; and with it I repeated the wave-length measures previously made with the resonator; indeed the column of air in the pipe in my hand responded so perfectly to the sounding-pipe that I thought it gave more marked results than those produced with the resonator.

The Manometric Flame-micrometer.

In the experiments described above we examined the appearances in the mirror with the unaided eye, and with it estimated when bisections and coincidences occurred; but to obtain results of precision, a method was devised which determines neatly these critical points. For that purpose I have invented the following

micrometer, founded on a beautiful suggestion of Dr. R. Radan, who thus describes, in his excellent *l'Acoustique* (Paris, 1867, p. 272), a method of observing the flames of two similar sounding organ-pipes :—" We attach to the two pipes two of König's flames arranged so that the point of one flame reaches above a small fixed mirror which hides its base, but which shows by reflection the base of the other flame. This produces the illusion of a single flame. If now we observe this hybrid image in the revolving mirror while we sound the two pipes, the point separates from the base, which proves that the two flames shine *alternately*, and the one retracts while the other elongates ; if the two tubes act on the same flame, the effect is *nil*, and the flame remains immoveable." By placing the above "small fixed mirror" on a divided circle, or by silvering its back and determining its angular displacements around a vertical axis by the method of Poggendorff (that is, by observing through a telescope the reflections of a fixed scale from the mirror), we have devised a simple and precise micrometer for ascertaining the amount of displacement of the resonator's flame. For, having once determined for a given note the amount of angular motion of the mirror required to move the bases of the flames over the distance between the centres of two contiguous serrations, we have the angular value of a displacement equal to that caused by moving the resonator through a wave-length ; and a fraction of the turn required to produce the above movement of the bases of the flames will be equal to that produced by the remove of the resonator over a corresponding fraction of a wave-length. Indeed, even with the unaided eye and without the use of the micrometric mirror, I have distinctly detected a displacement of the flames on moving the resonator (UT_3) over only 3 centimetres, or $\frac{1}{44}$ of its wave-length ; and with the micrometer I felt assured that I could determine the wave-surface of a body giving the note UT_3, to within 1 centimetre of its true position. Of course with higher notes we shall get a proportionally closer determination. But the object of this paper is not to present numerical results ; I reserve these for a subsequent communication, in which I will also present diagrams of apparatus and the appearances of the flames in various experiments.

I will here remark that the success of the experiments depends on the resonator with its attached tube being in perfect unison with the organ-pipe ; also the relative heights and positions of the flames should be so adjusted that the sharpest definitions are obtained in the rotating mirror, and so we are able to detect and measure the effects of small changes in the position of the resonator ; but these and other manipulative details will readily occur to any physicist who repeats the experiments.

Applications of the Method.

When the method I have here marked out shall have been reduced to the refinement it is susceptible of, I feel confident that we shall thereby have the means for attacking many problems of high theoretic interest which have heretofore been deemed beyond the reach of experiment. Its applications to such are so numerous that they are almost coextensive with the phenomena of sound.

The actual experimental determination of wave-surfaces in free air and in buildings can now certainly be accomplished; and such determinations may serve to extend our knowledge in the direction of giving the proper laws which should govern architects in their construction of rooms for public assemblies.

Without any consideration of the velocity of sound or of the number of vibrations pertaining to a given note, we can accurately measure the wave-length of the note by the following simple arrangement of apparatus. Take an organ-pipe and a resonator in unison with it, and place the resonator in a fixed position opposite the mouth of the pipe; then lead a gum tube from the resonator to a manometric capsule whose jet is contiguous to the jet of the organ-pipe, and adjust the flames to coincidence of serrations, using for that purpose the manometric micrometer. Now suppose, for simplicity, that the pipe gives 340 complete vibrations in one second; then, as the velocity of sound is 340 metres per second, it will take $\frac{1}{340}$ of a second for an aërial pulse to traverse 1 metre. Therefore, if the resonator-tube be lengthened $\frac{1}{2}$ metre, the serrations of its flame will no longer coincide with those of the pipe, but will bisect the spaces between the latter; for an impulse in the resonator-tube has now to travel such an increased length that it arrives at the manometric flame $\frac{1}{680}$ of a second later than before the tube was lengthened. If the tube be lengthened 1 metre, or a wave-length (English), the displacement of the resonator-flame will amount to the entire distance separating the centres of two contiguous serrations; and for n number of wave-length elongations of the tube we shall have n number of such displacements. Thus can be measured not only *one* but very *many* wave-lengths; for the intensities of the pulses I have not seen sensibly diminished after having traversed many metres of *firm* thick tubing; and hence, the errors of measurement being divided over so great a distance, the accuracy of the determination of a single wave-length is much increased, especially when the determinations are made in connexion with the manometric micrometer. If the number of vibrations given by the pipe can be determined with a proportionate accuracy, we shall succeed in arriving at precise measures of the *velocity*

($v = n\lambda$) of sound in different gases; for it is easy to close the mouth of the resonator with a delicate membrane, and fill the resonator, capsule, and connecting-tube with various gases; or we can substitute for the resonator a cavity of the proper volume and form, closed by a large membrane which vibrates in unison with the fundameutal note of the pipe, and proceed as above.

It requires but little consideration to see that the measure of a wave-length thus determined greatly exceeds in accuracy those obtained by the method heretofore practised, by which Dulong, Wertheim, and others have reached it indirectly, measuring the internodal distances in sounding pipes; and the above arrangement will, I think, give results even superior in precision to those which we can obtain by the use of the exquisite interference-apparatus which M. R. König has recently described in Poggendorff's *Annalen,* vol. cxlvi. p. 165[*].

In my lecture-room I have hung up before the students a series of gum tubes having lengths of $\frac{1}{2}$, 1, $1\frac{1}{2}$, 2, $2\frac{1}{2}$, 3, &c. wave-lengths of different notes. These are successively joined to the resonator and its manometric capsule, and they alternately produce in the flames coincidences and bisections of their series of serrations. Students after such exhibitions do not depart from the room with their usual scepticism as to the existence of a sonorous wave-length, but look upon the tubes as measures of actual entities.

The differences, if any, in the velocities of sounds corresponding to vibrations differing in intensity and frequency, may be determined by the use of reflectors and the direct observation of any changes in wave-length different from those which should take place on the assumption that notes of various intensities and heights are propagated with the same velocity.

Finally, we are bold enough to believe that we have in the highest development of the method a means of tracking in the air the resultant wave-surface of combined notes, and, in short, of bringing the exploration of acoustic space to approach somewhat to that precision of measurement which for over half a century has characterized the study of the ætherial vibrations producing light.

September 21st, 1872.

XL. *Notes on* Bessel's *Functions.* *By* the Hon. J. W. STRUTT, *late Fellow of Trinity College, Cambridge*[*].

THE value of Bessel's functions is becoming generally recognized. By means of them can be solved important problems in mathematical physics relating to the conduction of heat or electricity, the flow of an incompressible frictionless fluid which has been once at rest, and the vibrations of an elastic medium, when the nature of the question imposes conditions which are to be satisfied on spherical or cylindrical surfaces. Of late years the development of the subject has been mainly in the hands of German mathematicians.

In the present paper, which is of a somewhat fragmentary character, are contained some results of more or less novelty, some demonstrations of known theorems by what appear to me to be simple or more instructive methods, together with a few examples of applications to physical problems.

Different writers have started from different points in their investigations. So far as concerns its form, the Bessel's function of order n, $J_n(z)$, may be defined as that particular integral of the equation

$$\frac{d^2y}{dz^2} + \frac{1}{z}\frac{dy}{dz} + \left(1 - \frac{n^2}{z^2}\right)y = 0, \quad . \quad . \quad . \quad (1)$$

which remains finite when $z = 0$. The differential equation gives at once the ascending series

$$J_n(z) = \frac{z^n}{2^n\Gamma(n+1)}\left\{1 - \frac{z^2}{2(2n+2)} + \frac{z^4}{2.4.(2n+2)(2n+4)} + \dots\right\}, \quad (2)$$

with the exception of the arbitrary constant, which is determined by other considerations.

When n is integral, $J_n(z)$ may be expressed by the definite integral

$$J_n(z) = \frac{1}{\pi}\int_0^\pi \cos(z\sin\omega - n\omega)d\omega, \quad . \quad . \quad . \quad (3)$$

which is Bessel's original form. It readily appears that $J_n(z)$ can never be greater than unity, and, unless z and n are both small, is always much less.

The series in (2) never terminates for any value of n, but is always convergent for any value of n or z. However, when z is great, the convergence does not begin for a long time, and the series becomes useless for numerical calculation. In such cases another series proceeding by descending powers of z may be

* Communicated by the Author.

used with great advantage. We have

$$J_n(z) = \sqrt{\frac{2}{\pi z}} \left\{ 1 - \frac{(1^2 - 4n^2)(3^2 - 4n^2)}{1 \cdot 2 (8z)^2} + \ldots \right\} \cos\left(z - \frac{\pi}{4} - n\frac{\pi}{2}\right)$$

$$+ \sqrt{\frac{2}{\pi z}} \left\{ \frac{1^2 - 4n^2}{1 \cdot 8z} - \frac{(1^2 - 4n^2)(3^2 - 4n^2)(5^2 - 4n^2)}{1 \cdot 2 \cdot 3 (8z)^3} \right.$$

$$\left. + \ldots \right\} \sin\left(z - \frac{\pi}{4} - n\frac{\pi}{2}\right). \quad (4)*$$

If n be of the form $n =$ integer $+ \frac{1}{2}$, the series within brackets terminate, and we are furnished with expressions for $J_n(z)$ in a finite form. For instance, if $n = \frac{1}{2}$,

$$J_{\frac{1}{2}}(z) = \sqrt{\frac{2}{\pi z}} \cdot \sin z.$$

But if n be not of this form—for example, if n be integral,—the series run on to infinity, and become ultimately divergent, no matter how large z may be. Nevertheless the convergent part may be usefully employed in calculation; for it can be proved that the sum of any number of terms differs from the true value of the function by less than the last term included. The most satisfactory demonstration is that of Lipschitz (*Crelle*, vol. lvi.), who determines the remainder after any number of terms in the form cf a definite integral.

When z is extremely great, J_n reduces to

$$J_n(z) = \sqrt{\frac{2}{\pi z}} \cdot \cos\left(z - \frac{\pi}{4} - n\frac{\pi}{2}\right), \quad \cdot \quad \cdot \quad \cdot \quad (5)$$

becoming independent of n, except as to its phase. This might have been anticipated from (1).

The roots of the equations $J_n(z) = 0$, $J'_n(z) = 0$ are all real. When z is very great, they approximate respectively to

$$\left. \begin{array}{l} z = \dfrac{4m + 2n - 1}{4}\pi, \\[2mm] z = \dfrac{4m + 2n + 1}{4}\pi, \end{array} \right\} \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (6)$$

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* Series (4) was employed by Hansen in his calculations of Bessel's functions. Hansen's Tables will be found in Lommel, *Studien über die Bessel'schen Funktionen*, Leipzig. Mr. J. W. L. Glaisher (to whom I am indebted for the loan of Lommel) informs me that the B.A. Committee on Mathematical Tables, of which he is the Secretary, are about to turn their attention to the completion of the existing Tables of Bessel's functions. Convenient expressions for the calculation of J_n, and the roots of the equation $J_n = 0$, for $n = 0$, and (virtually) $n = 1$, are given in Professor Stokes's paper referred to below.

where m is an integer; but the smaller roots deviate greatly from these values.

If $\pm p$ denote the roots of $J_n(z) = 0$ (exclusive of zero),

$$J_n(z) = \frac{z^n}{2^n \Gamma(n+1)} \left\{ 1 - \frac{z^2}{p_1^2} \right\} \left\{ 1 - \frac{z^2}{p_2^2} \right\} \dots ; \quad . \quad (7)$$

whence, from (2),

$$\log \left\{ 1 - \frac{z^2}{2(2n+2)} + \frac{z^4}{2 \cdot 4 \cdot (2n+2)(2n+4)} \right.$$
$$\left. - \frac{z^6}{2 \cdot 4 \cdot 6 \cdot (2n+2)(2n+4)(2n+6)} + \dots \right\}$$
$$= \log \left\{ 1 - \frac{z^2}{p_1^2} \right\} + \log \left\{ 1 - \frac{z^2}{p_2^2} \right\} + \dots$$

Expanding both sides in rising powers of z, we find, on equating coefficients,

$$\Sigma \frac{1}{p^2} = \frac{1}{4(n+1)}.$$
$$\Sigma \frac{1}{p^4} = \frac{1}{16(n+1)^2(n+2)}. \quad \&c.$$

For example, if $n = 0$,

$$\Sigma \frac{1}{p^2} = \frac{1}{4}, \quad \Sigma \frac{1}{p^4} = \frac{1}{32}, \dots ;$$

and, if $n = 1$,

$$\Sigma \frac{1}{p^2} = \frac{1}{8}, \quad \Sigma \frac{1}{p^4} = \frac{1}{3 \cdot 64}.$$

In a similar manner, if $\pm q$ denote the roots of $J'_n(z) = 0$ (n not being zero),

$$\Sigma \frac{1}{q^2} = \frac{n+2}{4n(n+1)} \quad \&c.$$

If $n = 0$,

$$\Sigma \frac{1}{q^2} = \frac{1}{8}.$$

It should be noticed that a well-known theorem is included as a particular case. If $n = \frac{1}{2}$, $J_{\frac{1}{2}} \propto \sin z$, and $\Sigma \frac{1}{p^2} = \frac{1}{6}$. Since we know that the roots are of the form $\pm m\pi$, we infer that

$$\frac{1}{\pi^2} \Sigma \frac{1}{m^2} = \frac{1}{6}.$$

Very simple relations exist between the functions of neighbouring orders. As I shall have occasion to refer to them, I

state them here. They may easily be proved from the ascending series.

$$2J'_n = J_{n-1} - J_{n+1},$$
$$\frac{2n}{z}J_n = J_{n-1} + J_{n+1}, \quad \Big\} \quad \cdots \quad (8)$$

whence may also be derived

$$J'_n = \frac{n}{z}J_n - J_{n+1}. \quad \cdots \quad \cdots \quad (9)$$

The simpler forms approximated to, as z increases relatively to n, should be noticed.

One of the important applications of Bessel's functions is to the investigation of aërial vibrations in cylindrical spaces, the motion being perpendicular to the axis of the cylinder. The general differential equation governing the small vibrations is

$$\frac{d^2\phi}{dt^2} = a^2\Big(\frac{d^2\phi}{dx^2} + \frac{d^2\phi}{dy} + \frac{d^2\phi}{dz^2}\Big), \quad \cdots \quad (10)$$

where ϕ is the velocity-potential (assumed to exist). If we transform to polar coordinates in the plane xy, there results

$$\frac{d^2\phi}{dt^2} = a^2\Big(\frac{d^2\phi}{dr^2} + \frac{1}{r}\frac{d\phi}{dr} + \frac{1}{r^2}\frac{d^2\phi}{d\theta^2} + \frac{d^2\phi}{dz^2}\Big). \quad \cdots \quad (11)$$

If now the motion be independent of z, and of such a period that $\frac{2\pi}{\lambda} = \kappa$, we have (omitting the time factor)

$$\frac{d^2\phi}{dr^2} + \frac{1}{r}\frac{d\phi}{dr} + \frac{1}{r^2}\frac{d^2\phi}{d\theta^2} + \kappa^2\phi = 0. \quad \cdots \quad (12)$$

Suppose that we are considering the motion that can proceed within a rigid cylinder of radius $r = 1$. Whatever it may be, it can be divided into simple vibrations of various periods, each satisfying an equation like (12). Again, ϕ in (12) can by Fourier's theorem be expanded in a series proceeding by sines and cosines of multiples of θ. Considering the term containing $\cos(n\theta + \alpha)$, substituting in (12) and dropping the factor containing θ, we find

$$\frac{d^2\phi}{dr^2} + \frac{1}{r}\frac{d\phi}{dr} + \Big(\kappa^2 - \frac{n^2}{r^2}\Big)\phi = 0, \quad \cdots \quad (13)$$

which, on division by κ^2, appears in the form (1). The general integral of (13) may be written

$$\phi = AJ_n(\kappa r) + B \text{ (another function of } r),$$

but the function multiplied by B becomes infinite when r vanishes. In point of fact the function in question does not, pro-

perly speaking, satisfy equation (10) in passing through $r=0$. The solution of (12) may therefore be written

$$\phi = \Sigma\Sigma J_n(\kappa r) \cos(n\theta + \alpha),$$

the one summation relating to κ and the other to n. We have now to satisfy the boundary condition, that $\dfrac{d\phi}{dr}=0$ when $r=1$. It appears that only such values are admissible for κ as make $J'_n(\kappa)=0$. Reflecting now that *initially* ϕ may have any value within the circle $r=1$, we infer that any function of r may be expanded from $r=0$ to $r=1$ in a series of the form

$$A_1 J_n(\kappa_1 r) + A_2 J_n(\kappa_2 r) + \ldots,$$

κ_1, κ_2, ... being roots of $J'_n(\kappa)=0$. A similar argument would apply if κ were a root of $J_n(\kappa)=0$. A rigorous analytical proof of the possibility of these expansions would probably be difficult. The values of the coefficient we shall see how to determine presently.

Let us consider the functions

$$\left.\begin{array}{l} \phi = \epsilon^{-\kappa z} \cos n\theta \, J_n(\kappa r), \\ \phi' = \epsilon^{-\kappa' z} \cos n\theta \, J_n(\kappa' r). \end{array}\right\} \quad . \quad . \quad . \quad . \quad (14)$$

They satisfy Laplace's equation. When $z=0$ they become $\phi = \cos n\theta . J_n(\kappa r)$, $\phi' = \cos n\theta \, J_n(\kappa' r)$, respectively, and they vanish when $z=\infty$. If S denote the space bounded by $z=0$, $z=\infty$, and the cylinder $r=r$, we have, by Green's theorem,

$$\iint \phi \frac{d\phi'}{dn} dS = \iint \phi' \frac{d\phi}{dn} dS.$$

Over the plane end $z=0$,

$$\phi = \cos n\theta \, J_n(\kappa r), \quad \frac{d\phi'}{dn} = +\kappa' \cos n\theta \, J_n(\kappa' r);$$

over the cylindrical surface,

$$\phi = \epsilon^{-\kappa z} \cos n\theta \, J_n(\kappa r), \quad \frac{d\phi'}{dr} = \kappa' \epsilon^{-\kappa' z} \cos n\theta \, J'_n(\kappa r);$$

whence

$$\iint \phi \frac{d\phi'}{dn} dS = \int_0^{2\pi}\int_0^r r d\theta dr \cos^2 n\theta . \kappa' . J_n(\kappa r) J_n(\kappa' r)$$

$$+ \int_0^{2\pi}\int_0^\infty r d\theta \, dz \epsilon^{-(\kappa+\kappa')z} \cos^2 n\theta . \kappa' J_n(\kappa r) J'_n(\kappa' r)$$

$$= \pi\kappa' \int_0^r r dr J_n(\kappa r) J_n(\kappa' r) + \frac{\pi r \kappa'}{\kappa + \kappa'} J_n(\kappa r) . J_n(\kappa' r).$$

Therefore, by Green's theorem,

$$(\kappa' - \kappa) \int_0^r r dr \mathrm{J}_n(\kappa r) \mathrm{J}_n(\kappa' r)$$

$$+ \frac{r}{\kappa + \kappa'} \left\{ \kappa' \mathrm{J}_n(\kappa r) \mathrm{J}'_n(\kappa' r) - \kappa \mathrm{J}'_n(\kappa r) \mathrm{J}_n(\kappa' r) \right\} = 0. \quad . \quad (15)$$

Thus, *if κ and κ' are different,* and such that

$$\kappa' \mathrm{J}_n(\kappa) \mathrm{J}'_n(\kappa') - \kappa \mathrm{J}'_n(\kappa) \mathrm{J}_n(\kappa') = 0, \quad . \quad . \quad . \quad (16)$$

we have

$$\int_0^1 r dr \mathrm{J}_n(\kappa r) \mathrm{J}_n(\kappa' r) = 0. \quad . \quad . \quad . \quad . \quad . \quad (17)$$

The equation (16) may be satisfied in several ways. First, we may have $\mathrm{J}_n(\kappa)$, $\mathrm{J}_n(\kappa')$ equal to zero, so that κ, κ' are roots of $\mathrm{J}_n(z) = 0$; or κ, κ' may be roots of $\mathrm{J}'_n(z) = 0$; or lastly, they may be roots of the more general equation

$$\mathrm{M}z\mathrm{J}'_n(z) + \mathrm{N}\mathrm{J}_n(z) = 0, \quad . \quad . \quad . \quad (18)$$

which has an application in the theory of heat. In any of these cases (17) holds good.

If $\kappa' = \kappa$, (15), as it stands, becomes identical. We must take $\kappa' = \kappa + \delta\kappa$, and seek the limiting form as $\delta\kappa$ tends to vanish. Thus

$$2 \int_0^r r dr [\mathrm{J}_n(\kappa r)]^2$$

$$= \text{limit of } \frac{r \left\{ \kappa \mathrm{J}'_n(\kappa r) \mathrm{J}_n(\kappa + \delta\kappa) r - (\kappa + \delta\kappa) \mathrm{J}_n(\kappa r) \mathrm{J}'_n(\kappa + \delta\kappa) r \right\}}{\kappa \delta\kappa}$$

$$= \frac{r}{\kappa} \left\{ \kappa r [\mathrm{J}'_n(\kappa r)]^2 - \mathrm{J}_n(\kappa r) [\mathrm{J}'_n(\kappa r) + \kappa r \mathrm{J}''_n(\kappa r)] \right\}.$$

Accordingly

$$2 \int_0^1 r dr [\mathrm{J}_n(\kappa r)]^2 = [\mathrm{J}'_n(\kappa)]^2 - \mathrm{J}_n(\kappa) \left[\mathrm{J}''_n(\kappa) + \frac{1}{\kappa} \mathrm{J}'_n(\kappa) \right].$$

But by (1),

$$\mathrm{J}''_n(\kappa) + \frac{1}{\kappa} \mathrm{J}'_n(\kappa) = - \left(1 - \frac{n^2}{\kappa^2} \right) \mathrm{J}_n(\kappa),$$

so that

$$2 \int_0^1 r dr [\mathrm{J}_n(\kappa r)]^2 = [\mathrm{J}'_n(\kappa)]^2 + \left(1 - \frac{n^2}{\kappa^2} \right) [\mathrm{J}_n(\kappa)]^2. \quad . \quad (19)$$

(19) holds good for any value of κ; but in special cases it assumes

a simpler form. If κ be a root of $J_n(z)=0$, we have

$$2\int_0^1 r\,dr\,[J_n(\kappa r)]^2=[J'_n(\kappa)]^2=[J_{n+1}(\kappa)]^2 \quad . \quad . \quad (20)$$

in virtue of (9).

If κ be a root of $J'_n(z)=0,$

$$2\int_0^1 r\,dr\,[J_n(\kappa r)]^2=\left(1-\frac{n^2}{\kappa^2}\right)[J_n(\kappa)]^2=-J_n(\kappa)\,J''_n(\kappa). \quad (21)$$

In particular, if $n=0,$

$$2\int_0^1 r\,dr\,[J_0(\kappa r)]^2=[J_0(\kappa)]^2.$$

More generally, if κ be a root of (18),

$$2\int_0^1 r\,dr\,[J_n(\kappa r)]^2=\left\{1+\frac{1}{\kappa^2}\left(\frac{N^2}{M^2}-n^2\right)\right\}[J_n(\kappa)]^2$$

$$=\left\{1+(\kappa^2-n^2)\frac{M^2}{N^2}\right\}[J'_n(\kappa)]^2. \quad . \quad . \quad . \quad (22)$$

Results corresponding to equations (17) and (22) are given by Fourier (*Théorie de la Chaleur*, Chap. VI.) for the special case of $n=0$. The extension to the general value of n (I am not sure that he contemplates fractional values) is due to Lommel; but he drops generality in another direction by confining himself to the case where κ satisfies $J_n(\kappa)=0$. His results are accordingly equivalent to (17) and (20). So far as I am aware, the general equations (17), (18), (22) are new.

It should be noticed that the same method is applicable to the hollow cylinder bounded by $r=r_1,\ r=r_2$, provided that instead of J_n the *complete* integral of (1) is used. In general the form will be AJ_n+BJ_{-n}, where A and B are arbitrary constants; but if n be integral, J_n must be replaced by a more complicated function denoted by Y_n (see Lommel). The complete primitive *may* be used, because the space through which Green's theorem is to be applied does not include the axis $r=0$; it *must* be used in order to get a general solution, because there will now be two boundary conditions to be satisfied. If $f_n(\kappa r)$ be the complete integral, and $\kappa,\ \kappa'$ are subject to the conditions

$$M_1\kappa f_n'(\kappa r_1)+N_1 f_n(\kappa r_1)=0,$$
$$M_2\kappa f_n'(\kappa r_2)+N_2 f_n(\kappa r_2)=0,$$

with similar equations for κ', the equivalent of (17) stands

$$\int_{r_1}^{r_2} f_n(\kappa r)f_n(\kappa'r)r\,dr=0,$$

always provided that κ and κ' are different. The arbitrary constants contained in the expression of f_n allow of an indefinite

number of values of κ satisfying the pair of boundary conditions written above, whatever may be the values of M_1, M_2, N_1, N_2 for any assigned radii r_1 and r_2. In certain cases it may happen that either A or B vanishes, and that only one of the component functions is required. It is scarcely necessary to say that, from the more general case of a hollow cylinder, the formulæ appropriate to a complete cylinder may be derived by simply putting $r_1 = 0$.

In the formation of equation (14) we have tacitly assumed that, after a complete circuit from $\theta = 0$ to $\theta = 2\pi$, the functions ϕ, ϕ' recur. But if n be fractional, this will not be true; and in applying Green's theorem, we must take account of the fact by including the surface-integrals over the planes $\theta = 0$, $\theta = 2\pi$, which now no longer destroy one another. However the additional integrals are, as is easily seen, symmetrical in κ and κ', and therefore disappear from equation (15). Accordingly the results remain precisely as before.

Previously to applying (17) and (22) to the expansion of an arbitrary function of r in a series of Bessel's functions of order n, a certain restriction should be noticed. The necessity of it will appear from the consideration that if r is indefinitely small, $J_n(\kappa r)$ contains no lower power of r than r^n, showing that the arbitrary function must possess the same peculiarity. This circumstance, however, does not interfere with the generality of the expansion of an arbitrary continuous function of two variables within the circle $r = 1$. Let the function be expressed by rectangular coordinates x and y, and expand it by Taylor's theorem in rising powers of those variables. On substitution of polar coordinates, any power of r, say r^n, will be accompanied by powers of $\sin \theta$ and $\cos \theta$ not higher than the nth; or, when these again are expressed by sines and cosines of multiple arcs, the coefficient of θ will not arise above n. It follows that when a continuous function is expanded by Fourier's theorem in the manner supposed, the coefficient of $\cos(n\theta + \alpha)$, considered as a function of r, will contain no lower power than r^n.

Suppose now that $f(r)$, subject to the above restriction, is expanded in the form
$$f(r) = \Sigma A_p J_n(\kappa_p r), \quad \ldots \quad (23)$$
where κ_p is a root of (18). If we multiply both sides by $J_n(\kappa_p r) \cdot r$, and integrate with respect to r from $r = 0$ to $r = 1$, we have by (17) and (19),

$$2 \int_0^1 r\,dr f(r) J_n(\kappa_p r) = A_p \int_0^1 2r\,dr [J_n(p_\kappa r)]^2$$
$$= A_p \left\{ [J'_n(\kappa)]^2 + \left(1 - \frac{n^2}{\kappa^2}\right) [J_n(\kappa)]^2 \right\};$$

whence

$$A_p = 2\int_0^1 rdrf(r)J_n(\kappa r) \div [J'_n(\kappa)]^2 + \left(1 - \frac{n^2}{\kappa^2}\right)[J_n(\kappa)]^2. \quad (24)$$

In particular, if κ be a root of $J_n(\kappa) = 0$,

$$A_p = 2\int_0^1 rdrf(r)J_n(\kappa r) \div [J'_n(\kappa)]^2;$$

or if κ be a root of $J'_n(\kappa) = 0$, the same integral must be divided by

$$\left(1 - \frac{n^2}{\kappa^2}\right)[J_n(\kappa)]^2 \text{ or } -J_n(\kappa) \cdot J'_n(\kappa).$$

An application of these formulæ will be found below.

If $f(r)$ does not fulfil the above specified condition, but remains finite when $r = 0$, we may, following Lommel, write

$$f(r) = \Sigma A r^{-n} J_n(\kappa r),$$

whence

$$2\int_0^1 r^{n+1} f(r) J_n(\kappa r) dr = A \int_0^1 2r dr [J_n(\kappa r)]^2,$$

as before.

The application to the problem of vibration in two dimensions is very easy. The particular solution of (12) is

$$\phi = \cos(\kappa at + \epsilon) \cdot J_n(\kappa r) \cdot \cos(n\theta + \alpha), \quad . \quad . \quad (25)$$

κ being determined from (18) or one of its special forms according to the circumstances of the case. The most interesting in the application to acoustics is when the cylinder $r = 1$ is rigid, so that $J'_n(\kappa) = 0$. The lower values of κ (calculated from Hansen's Tables by means of the relations allowing J_n to be expressed in terms of J_0 and J_1) are given in the following Table :—

Order of Harmonic.

		0.	1.	2.	3.
Number of internal circular nodes.	0	3·832	1·841	3·054	4·201
	1	7·015	5·332	6·705	8·015
	2	10·174	8·536	9·965	11·344
	3	13·324	11·706		
	4	16·471	14·864		
	5	19·616	18·016		

When κ is very great the roots are given by (6), the series being the same for the alternate functions. The trouble of the calculation of the earlier roots increases rapidly with n. When n is great, it would appear probable from physical considerations that the first root varies as n; but I have not succeeded in put-

ting the functions under any simple form when n is very great. Both the ascending and descending series fail in such a case if z is comparable with n. Light is thrown on the question by a consideration of the differential equation (1) itself. The ascending series is founded on the supposition that the coefficient multiplying y, really $\left(1 - \dfrac{n^2}{z^2}\right)$, can be approximately represented by $-\dfrac{n^2}{z^2}$; if the term 1 be absolutely neglected, the solution is

$$y \propto z^n.$$

On the other hand, the descending series has its foundation in a substitution of $\frac{1}{2}$ for n, when the exact solution becomes

$$y \propto J_{\frac{1}{2}}(z) \propto z^{-\frac{1}{2}} \sin z.$$

When, however, n is great and z comparable with it, neither term in $1 - \dfrac{n^2}{z^2}$, over a large range of z, can be treated as relatively unimportant. For a considerable range in the neighbourhood of n the differential equation approximates to

$$\frac{d^2y}{dz^2} + \frac{1}{z}\frac{dy}{dz} = 0,$$

of which the solution would be

$$y = A + B \log z.$$

The question is worthy of the attention of analysts; for Bessel's functions with large values of n (1000 or more) would have physical applications, for instance, to the problem of the rainbow.

In virtue of the integral formulæ, a combination of the partial solutions (25) can be found to correspond to any initial values of ϕ and $\dfrac{d\phi}{dt}$.

The problem becomes only a little more complicated if we suppose the cylinder closed at $z = 0$ and $z = l$, and discard the restriction that the motion shall be independent of z. The particular solution is

$$\phi = \cos\left(\kappa a t + \epsilon\right)\cos\left(p\,\frac{\pi z}{l}\right)\cos\left(n\theta + \alpha\right)J_n\left\{\left(\kappa^2 - p^2\frac{\pi^2}{l^2}\right)^{\frac{1}{2}} r\right\}, \quad (26)$$

p being an integer. For a rigid cylinder of radius unity we have

$$\kappa^2 = p^2\frac{\pi^2}{l^2} + K^2, \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (27)$$

where K denotes the values corresponding to $p = 0$, being those given in the Table. To every value of K corresponds a series of values of κ, found by ascribing to p in succession the values

1, 2, 3, ... The purely axial vibrations correspond to a zero value of K. A similar analysis would apply on Euler and Lagrange's hypothesis if the ends or the side of the cylinder were open, though in the latter case the result would be no approximation to the truth. At the plane ends we may legitimately take as an approximation $\phi = 0$, provided that the radius of the cylinder bears but a small ratio to the wave-length of the vibration under consideration. (See a paper "On Resonance," Phil. Trans. 1871.)

In the problem, partially considered by Fourier (*Théorie Analytique de la Chaleur*, Chap. VI.), of a cylinder of uniformly conducting material heated arbitrarily and then allowed to cool by radiation, the cosine factor containing the time would be replaced by an exponential, the coefficient of t being negative, and (18) would have to be used as boundary condition, but otherwise there would be little change.

Hitherto we have had to do with integral values of n only; but if instead of the complete cylinder we take the sector cut off by $\theta = 0$, $\theta = \beta$, fractional values will be introduced. The elementary solution for the acoustical problem in two dimensions then becomes

$$\phi = \cos (\kappa a t + \epsilon) \cos n\theta . J_n(\kappa r), \quad . \quad . \quad . \quad (28)$$

where

$$n = (m = \text{integer}) \times \frac{\pi}{\beta}. \quad . \quad . \quad . \quad . \quad . \quad (29)$$

Hence, as might have been foreseen, if β be an aliquot part of π (or π itself), the complete solution requires only integral values of n; but otherwise functions of fractional orders must be introduced. An interesting example is when $\beta = 2\pi$, corresponding to a cylinder with a rigid partition from the centre to the circumference. When m is even, n becomes integral, and we have motions which might take place without the partition, and therefore presenting nothing peculiar for consideration; but if m is odd, n assumes the form (integer $+ \frac{1}{2}$), in which case, as we have seen, J_n is expressible in finite terms. Thus if $m = 1$, $n = \frac{1}{2}$, and

$$\phi = \cos (\kappa a t + \epsilon) \cos \frac{\theta}{2} J_{\frac{1}{2}}(\kappa r) \propto \cos(\kappa a t + \epsilon) \cos \frac{\theta}{2} . (\kappa r)^{-\frac{1}{2}} \sin \kappa r. (30)$$

The admissible values of κ are those which render $\tan \kappa$ equal to 2κ. The first value of κ is $1 \cdot 1655$, giving a much lower tone than any of which the complete cylinder is capable.

If instead of (14) we were to take

$$\left. \begin{array}{l} \phi = \epsilon^{-z} \cos n\theta J_n(r), \\ \phi' = \epsilon^{-z} \cos m\theta J_m(r), \end{array} \right\} \quad . \quad . \quad . \quad . \quad (31)$$

we should find by an application of Green's theorem to the space bounded by $z=0$, $z=\infty$, $r=r$, $\theta=0$, $\theta=\alpha$:—

$$r\{J_n.J'_m - J'_n.J_m\} = (m^2-n^2)\int_0^r J_n.J_m.\frac{dr}{r}. \quad . \quad (32)$$

In a similar manner, by use of

$$\left.\begin{aligned}\phi &= \epsilon^{-z}\cos n\theta J_n(r),\\ \phi' &= r^m \cos m\theta,\end{aligned}\right\} \quad . \quad . \quad . \quad . \quad (33)$$

another useful formula may be arrived at. But it is perhaps simpler to proceed from the differential equation (1) itself.

$$\int_0^r dr.r^{m+1}\left\{J''_n + \frac{1}{r}J'_n + \left(1-\frac{n^2}{r^2}\right)J_n\right\} = 0.^1$$

Now

$$\int_0^r r^m dr(rJ''_n + J'_n) = \int r^m d(rJ'_n) = r^{m+1}J'_n - m\int_0^r r^m J'_n dr$$

$$= r^{m+1}J'_n - m\left\{r^m J_n - m\int_0^r r^{m-1}J_n dr\right\}.$$

Thus

$$\int_0^r r^{m+1}J_n dr = mr^m J_n - r^{m+1}J'_n + (n^2-m^2)\int_0^r r^{m-1}J_n dr \quad (34)$$

—a formula of reduction, by means of which, if m be even, the integral on the left may be evaluated.

If $m=n$, we have

$$\int_0^r r^{n+1}J_n dr = nr^n J_n - r^{n+1}J'_n = r^{n+1}J_{n+1} \text{ by (9),}$$

a formula given by Lommel, p. 20.

If $n=0$,

$$\int_0^r r^{m+1}J_0(r)dr = mr^m J_0(r) - r^{m+1}J'_0(r) - m^2\int_0^r r^{m-1}J_0(r)dr. \quad (35)$$

Thus

$$\int_0^r r\,dr J_0(r) = -rJ'_0(r) = rJ_1(r),$$

$$\int_0^r r^3 dr J_0(r) = 2r^2 J_0 - r^3 J'_0 + 4rJ'_0,$$

$$\int_0^r r^5 dr J_0(r) = (4r^4 - 32r^2)J_0 + (-r^5 + 16r^3 - 64r)J'_0, \&c.$$

If r is a root of $J'_0(r)=0$, the three integrals become simply

$$0, \quad 2r^2 J_0(r), \quad 4r^2(r^2-8)J_0(r).$$

In the theory of the vibrations of a gas, or the flow of heat within a sphere, the notation of Bessel's functions may advantageously be introduced. The expansion in Spherical Harmonics replaces the series of Fourier, and instead of (13) the equation satisfied by the coefficient of S_n becomes

$$\frac{d^2\phi}{dr^2} + \frac{2}{r}\frac{d\phi}{dr} - \frac{n(n+1)}{r^2}\phi + \kappa^2\phi = 0, \quad . \quad . \quad (36)$$

which may be also written

$$\frac{d^2(r\phi)}{dr^2} + \left(\kappa^2 - \frac{n(n+1)}{r^2}\right)r\phi = 0. \quad . \quad . \quad . \quad (37)$$

Equation (13), however, may be put into the form

$$\frac{d^2(r^{\frac{1}{2}}\phi)}{dr^2} + \left(\kappa^2 - \frac{4n^2-1}{4r^2}\right)r^{\frac{1}{2}}\phi = 0; \quad . \quad . \quad (38)$$

from which we see that, since the solution of (38) (subject to the condition at $r=0$) is

$$\phi = J_n(\kappa r),$$

or

$$r^{\frac{1}{2}}\phi = r^{\frac{1}{2}}J_n(\kappa r),$$

the solution of (37) under a like condition must be

$$r\phi = r^{\frac{1}{2}}J_{n+\frac{1}{2}}(\kappa r)$$

or

$$\phi = r^{-\frac{1}{2}}J_{n+\frac{1}{2}}(\kappa r) \cdot S_n, \quad . \quad . \quad . \quad . \quad (39)$$

if the angular factor be restored. $J_{n+\frac{1}{2}}$ is, as we have seen, expressible in finite terms; but it is not easy thence to derive the approximate form when r is small. As it is, the known theorems about Bessel's functions allow us to expand (39) at once in an ascending series. For the problem of vibrations in a rigid sphere, we require the roots of $\dfrac{d\phi}{dr} = 0$; or if $r=1$,

$$J_{n+\frac{1}{2}}(\kappa) = 2\kappa J'_{n+\frac{1}{2}}(\kappa). \quad . \quad . \quad . \quad . \quad (40)$$

An investigation of this problem will be found in the Mathematical Society's 'Proceedings' for 1872.

For the conduction question, we have a linear relation between ϕ and $\dfrac{d\phi}{dr}$ when $r=1$, say,

$$M'\phi + N'\frac{d\phi}{dr} = 0.$$

Hence from (39), when $r=1$,

$$(M' - \tfrac{1}{2}N')J_{n+\frac{1}{2}}(\kappa) + N'\kappa J'_{n+\frac{1}{2}}(\kappa), \quad . \quad . \quad . \quad (41)$$

which comes under the form (18).

The compounding of particular solutions like (39) to represent given arbitrary values of ϕ and $\dfrac{d\phi}{dt}$ (or, in the heat problem, of ϕ merely) presents no difficulty after what precedes. Fourier himself has given the complete solution for the case of $n=0$, when $J_{n+\frac{1}{2}}(z) \propto \dfrac{\sin z}{z^{\frac{1}{2}}}$; but I am not aware that the problem has been considered before in its generality.

The problem appended forms a good example of the application of Bessel's functions to a special case, in which a numerical result is required; it was not invented for the purpose, but presented itself in the course of an acoustical investigation two or three years ago. At the time I was not able to give a solution.

A rigid cylinder contains incompressible fluid which has been once at rest, and is set in motion in such a manner that at a certain section (perpendicular to the axis) the velocity parallel to the axis is expressed by $1+\mu r^2$, where r is the distance of any point from the centre of the section. It is required to determine the motion and the energy thereof.

Taking the axis of the cylinder for that of z, and $z=0$ for the plane section, while $r=1$ is the equation of the cylinder, we have the following conditions to which the potential ϕ is subject:—

(1) that when $r=1$, $\dfrac{d\phi}{dr}=0$ for all positive z;

(2) that when $z=0$, $\dfrac{d\phi}{dz}=1+\mu r^2$, from $r=0$ to $r=1$.

The rate of total flow across $z=0$ is

$$\int_0^r 2\pi r dr(1+\mu r^2)=\pi(1+\tfrac{1}{2}\mu).$$

Let ϕ_0 correspond to this distributed uniformly, so that

$$\frac{d\phi_0}{dz}=1+\tfrac{1}{2}\mu \text{ and } \phi_0=(1+\tfrac{1}{2}\mu)z.$$

For the remaining part of ϕ we have

$$\frac{d\phi_1}{dz}=\mu(r^2-\tfrac{1}{2}).$$

Assume for ϕ_1

$$\phi_1=\Sigma a_p \epsilon^{-pz}J_0(pr),$$

where p is a root of the equation

$$J'_0(p)=0.$$

Each term in the expression for ϕ_1 satisfies Laplace's equation and the condition laid down for the cylindrical boundary, while it vanishes when z becomes infinitely great.

The first value of p is zero; but we have already included the corresponding terms in ϕ_0. The next values are

3·8318	13·324
7·0152	16·471
10·1735	19·616,

approximating to the form

$$(m+\tfrac{1}{4})\pi - \cdot 151982 \,\frac{\pi}{4m+1}\,{}^*.$$

When $z=0$,

$$\frac{d\phi_1}{dz} = -\Sigma p a_p \mathrm{J}_0(pr) \,;$$

so that the condition to be satisfied from $r=0$ to $r=1$ is

$$-\Sigma p a_p \mathrm{J}_0(pr) = \mu(r^2 - \tfrac{1}{2}).$$

Multiplying both sides by $\mathrm{J}_0(pr)$ and integrating from 0 to 1, every term but one on the left vanishes, and we have by the theorem

$$p a_p \mathrm{J}_0(p) \cdot \mathrm{J}''_0(p) = 2\mu \int_0^1 r\,dr\,\mathrm{J}_0(pr)\,(r^2 - \tfrac{1}{2}) \,;$$

or, since from (1)

$$\mathrm{J}''_0(p) + \mathrm{J}_0(p) = 0,$$

$$-p a_p [\mathrm{J}_0(p)]^2 = 2\mu \int_0^1 r\,dr\,\mathrm{J}_0(pr)(r^2 - \tfrac{1}{2}).$$

Now, on the right-hand side,

$$\int_0^1 r\,dr\,\mathrm{J}_0(pr) = \frac{1}{p^2}\int_0^p x\,dx\,\mathrm{J}_0(x) = \frac{-1}{p^2}\int_0^p dx\,\{x\mathrm{J}''_0(x) + \mathrm{J}'_0(x)\}$$

$$= \frac{-1}{p^2}\big[\,p\mathrm{J}'_0(p)\,\big] = 0.$$

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* See a paper by Professor Stokes in the Cambridge Transactions, vol. ix., "On the Numerical Calculation of Definite Integrals and Infinite Series." In example III. the integral $\int_0^x x\,dx\,\mathrm{J}_0(x)$ is considered under the notation v. Now, since $\mathrm{J}''_0 + \frac{1}{x}\mathrm{J}'_0 + \mathrm{J}_0 = 0$, it follows that

$$\int_0^x x\,dx\,\mathrm{J}_0(x) = -x\mathrm{J}'_0(x) = x\mathrm{J}_1(x) \,;$$

so that the roots of the equation $v=0$, given by Stokes, are none other than the roots of $\mathrm{J}'_0(x)=0$, or $\mathrm{J}_1(x)=0$.

Again, by the formula of reduction,

$$\int_0^r r^{m+1} J_n(r) dr = m r^m J_n(r) - J'_n(r) \cdot r^{m+1}$$
$$+ (n^2 - m^2) \int_0^r r^{m-1} J_n(r) dr ;$$

putting $n = 0$, $m = 2$, we find

$$\int_0^r r^3 J_0(r) dr = 2 r^2 J_0(r) - r^3 J'_0(r) - 4 \int_0^r r dr J_0(r) ;$$

or, taking p as the upper limit of integration,

$$\int_0^p r^3 J_0(r) dr = 2 p^2 J_0(p).$$

Thus

$$\int_0^1 r^3 dr J_0(pr) = \frac{1}{p^4} \cdot 2 p^2 J_0(p) = \frac{2}{p^2} J_0(p).$$

Accordingly, to determine a_p, we have

$$-p a_p [J_0(p)]^2 = \frac{4\mu}{p^2} J_0(p),$$

whence

$$a_p = -\frac{4\mu}{p^3 J_0(p)}; \quad a_p^2 = \frac{16\mu^2}{p^6 [J_0(p)]^2}.$$

The complete value of ϕ is thus

$$\phi = (1 + \tfrac{1}{2}\mu) z - 4\mu \Sigma \frac{\epsilon^{-pz}}{p^3 J_0(p)} J_0(pr),$$

$$\frac{d\phi}{dz(=0)} = (1 + \tfrac{1}{2}\mu) + 4\mu \Sigma \frac{J_0(pr)}{p^2 J_0(p)} = 1 + \mu r^2.$$

If we put $r = 1$, we find

$$1 + \mu = 1 + \tfrac{1}{2}\mu + 4\mu \Sigma \frac{1}{p^2} ;$$

so that

$$\Sigma \frac{1}{p^2} = \frac{1}{8},$$

in accordance with what we have proved already.

The energy of the motion

$$= \tfrac{1}{2} \iiint \Sigma \left(\frac{d\phi}{dx}\right)^2 dx \, dy \, dz,$$
$$= \tfrac{1}{2} \int_0^1 2\pi r dr \phi \frac{d\phi}{dz} \text{ when } z = l = \infty,$$
$$- \tfrac{1}{2} \int_0^1 2\pi r dr \phi \frac{d\phi}{dz} \text{ when } z = 0,$$

by Green's theorem.

The first integral $= \pi l (1 + \frac{1}{2}\mu)^2$.

In forming the second, we must remember that if p_1 and p_2 are two different values of p,

$$\int_0^1 2\pi r \, dr \, \mathrm{J}_0(p_1 r) \mathrm{J}_0(p_2 r) = 0.$$

Thus

$$\int_0^1 2\pi r \, dr \, \phi \frac{d\phi}{dz} \text{ when } z = 0$$

$$= -16\mu^2 \int_0^1 2\pi r \, dr \, \Sigma \frac{[\mathrm{J}_0(pr)]^2}{p^5 [\mathrm{J}_0(p)]^2} = -16\mu^2 \pi \Sigma \frac{1}{p^5}.$$

Accordingly

$$2 \text{ Kinetic Energy } = \pi l (1 + \frac{1}{2}\mu)^2 + 16\pi\mu^2 \Sigma \frac{1}{p^5},$$

or

$$\frac{2 \text{ Kinetic Energy}}{[\text{Rate of Total Flow}]^2} = \frac{l}{\pi} + 16 \frac{\mu^2}{\pi(1 + \frac{1}{2}\mu)^2} \Sigma \frac{1}{p^5}.$$

By calculation I find for the approximate value of $\Sigma \dfrac{1}{p^5}$,

$$\Sigma \frac{1}{p^5} = \cdot 0012822.$$

In the application to the investigation which was the origin of the problem here considered,

$$16 \frac{\cdot 0012822 \, \mu^2}{(1 + \frac{1}{2}\mu)^2} + \frac{8}{3\pi} \frac{1 + \frac{14}{15}\mu + \frac{5}{21}\mu^2}{(1 + \frac{1}{2}\mu)^2}$$

has to be made a minimum by variation of μ.

The minimum value comes out

$$\alpha = \cdot 8281.$$

In the paper " On Resonance " (Phil. Trans. 1871) I had determined $\alpha = \cdot 8282$ by the consideration of a motion within the cylinder satisfying the same boundary conditions as in the present problem, but not having a velocity-potential. The close agreement of the results furnishes a confirmation of the methods used on that occasion.

XLI. *Fluorescent Relations of certain solid Hydrocarbons found in Coal-tar and Petroleum Distillates. By* HENRY MORTON, *Ph.D., President of the Stevens Institute of Technology*.*

IN the course of a general research on the fluorescent spectra, I have encountered certain facts in reference to anthracene and one of its associated bodies, and also with regard to a new solid hydrocarbon or mixture of hydrocarbons first observed and separated by me from certain petroleum distillates, which seem to merit a special notice.

It will, I think, avoid confusion first to discuss the substances first named, and afterwards point out the relations and properties of the second class of bodies.

Proceeding, then, to the consideration of anthracene, I will describe

The Materials used.—These consisted of :—1. Crude anthracene from L. C. Marquart, of Bonn. This is a dark olive-green pulverulent mass with fragments of a lighter green scattered in it. 2. Crude anthracene from the works of Page, Kidder, and Fletcher, Bull's Ferry, New Jersey, U.S. This was kindly furnished by Mr. J. C. F. Chever, Chemical Superintendent, and was in three forms: (*a*) fused, as a dark olive-green, hard, crystalline mass; (*b*) washed and pressed, much like that from Marquart, but of a lighter colour ; (*c*) in powder, darker and less free from tarry matter than the foregoing. 3. Chemically pure anthracene, supplied by Mr. Schering, of Berlin, a light yellow-brown crystalline powder. 4. A purer form, obtained by washing the preceding with cold alcohol, or by distilling 1, or 2 (*a*), or 2 (*b*) in a current of air, as indicated in the accompanying cut (fig. 1), and then washing with cold alcohol. 5. A yet purer form, obtained as above by distillation with great care and repeated crystallization from hot alcohol as recommended by Kopp (see *Moniteur Scientifique Quesneville*, Aug. 1872, p. 535). 6th. Absolutely chemically pure anthracene, obtained by exposure of the hot solution to sunlight and subsequent recrystallization.
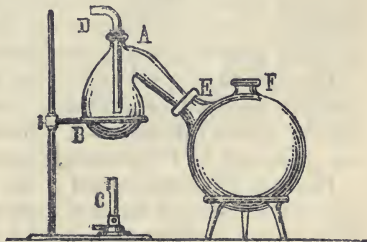
Fig. 1.

Method of observing the Fluorescent Spectrum.—This is essentially that described by Stokes as his first method (see Phil.

Trans. 1852, Part II. p. 469), and also used by Becquerel (see *La Lumière,* vol. i. p. 335, and *Comptes Rendus,* vol. lxxv. p. 297), as also by Hagenbach. Figure 2 hardly needs any explanation, except that A is a porte-lumière with a lens at B, and a small tank of ammonio-cupric sulphate in front, C a revolving stand to hold objects, and D a spectroscope.

Fig. 2.
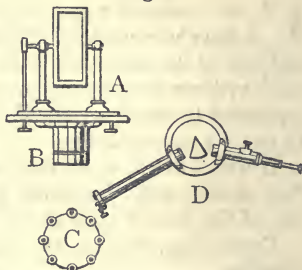
Observations on the Fluorescent Spectrum.—All the forms of anthracene except the last show, when observed as above, the same spectrum, which seems to me also to correspond as nearly as one would expect with the drawing given by Becquerel (in *La Lumière*) of a hydrocarbon having the colour of the uranium salts and obtained from Fritzsche, who first investigated what we now call anthracene. This spectrum is shown in fig. 3.

Fig. 3.
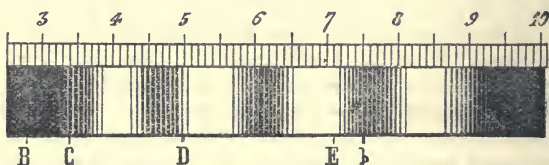
Spectrum of Impure Anthracene referred to Bunsen's Scale.— The anthracene described above as the sixth variety, however, shows a fainter fluorescence and gives a continuous spectrum. It yields, however, with the various solvents and chemical tests, abundant evidence that it is not para-anthracene, but simply a perfectly pure condition of anthracene itself.

It would thus appear that some part of the brilliant blue fluorescence ascribed to anthracene may have been due, as was beyond doubt the spectrum figured by Becquerel and also the one measured by Hagenbach, to an adherent impurity.

Cause of Fluorescence and of the Banded Spectrum of Commercial Anthracene.—A series of experiments make it clear that the yellow body persistently adhering to anthracene and soluble in ether, benzole, and carbon-bisulphide, as described by Fritzsche under the name chrysogen, was the substance in question. It is most abundant, and free from interfering substances, in the fourth sort of anthracene; for it seems to be practically insoluble in alcohol, the brown or yellow matter take up by that fluid being quite a different substance.

Fluorescent Spectrum of Solution.—When impure anthracene in the fourth condition is dissolved in benzole, it gives a bright-yellow solution fluorescing strongly with a light which appears green and yields, on analysis, a spectrum which closely resembles that of the solid, having, however, all its bands displaced towards the more refrangible end. This is shown in fig. 4.

Fig. 4.
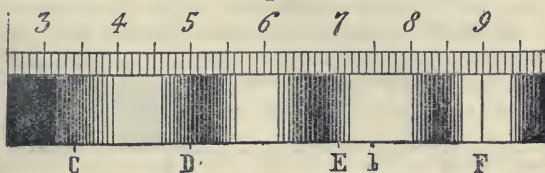
Spectrum of Chrysogen in Solution.—But I ought to say that in this, and also the preceding spectrum, the band at 6·1 of the scale here and 4·7 in the other, is represented in the engraving as too strong and broad, being in fact fainter and narrower than the other bands.

This displacement of fluorescent bands by solution finds a parallel in the case of a substance first observed by me in some petroleum-residues many months since (see 'Proceedings of Franklin Institute,' vol. lxiii. p. 296). This gives a fluorescent spectrum having a close relation to that of chrysogen, and, like it, showing a displacement upwards by solutions differing with the solvent.

Hagenbach, in his last paper, which has just reached me in due course of post (see Poggendorff's *Annalen*, 1872), announces that he has just observed a displacement of maxima in the fluorescent spectrum of some solutions by a change in the solvent.

Absorption-spectrum of Chrysogen.—When the same impure anthracene (4), if spread thinly on paper or mixed with paraffine, is spread on glass, or is fused between slips of mica, and is then viewed by transmitted blue light in the manner shown in fig. 5, we obtain a marked absorption-spectrum which

Fig. 5.

A is the port-lumière with a diaphragm at B, C is a tank containing a solution of ammonio-sulphate of copper, D an adjustable table carrying the solid or solution to be examined, and E a Browning one-prism spectroscope.

is very characteristic and may be recognized in all but the white forms (5 and 6).

This spectrum consists of a dark band about and above the Fraunhofer-line F, of another not quite so well defined and broader between F and G, and lastly a band about G, which continues into the absolute absorption commencing at 14 of scale (see fig. 6).

Fig. 6.
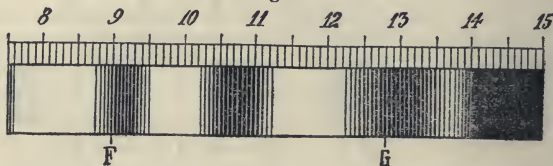
Fig. 7.
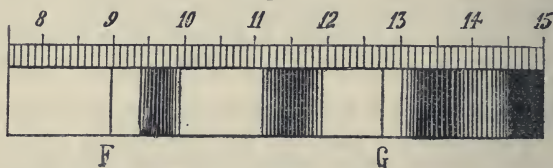
This absorption-spectrum has its bands displaced upwards by solution in the same manner as the fluorescent one; and the same is true of the new body obtained from petroleum above described*. This absorption (figs. 6 and 7) is undoubtedly due to chrysogen; for, among other facts, it rapidly disappears from a solution on exposure to sunlight. To see it clearly, the brown matter soluble in alcohol should be removed; for this exerts a general absorption of the entire spectrum above 10 of scale.

Maxima and Minima.—When a pure spectrum is thrown on a screen coated with either of these hydrocarbons, or on a tank filled with their solutions, a series of maxima and minima are observed corresponding exactly with the absorption-spectra of the same substances, a maximum of fluorescence coinciding with a band of absorption. Such a condition as this was entirely to be expected, and was observed by Stokes in solution of leaf-green (Phil. Trans. 1852, Pt. II. p. 491), in canary-glass, and in nitrate of uranium (Ib. pp. 497 and 517). Hagenbach has likewise remarked the same thing in many instances; but the completeness of this relation, changing with the change due to solution, makes these examples specially interesting.

I would also here remark that the analogy between anthra-

* I have observed also a displacement downwards of the absorption-bands of oxalate of uranium by solution.

cene and this new body which I have found in petroleum residues does not cease here.

If this latter is exposed in hot solution in benzole to strong sunlight for many hours, it deposits, on cooling, needle-like crystals which are almost colourless, and give a spectrum by fluorescence which corresponds very closely with that of the solution of the same body mentioned already. Its bands, however, are far less strongly marked than those of the unsolarized material; and I have little doubt that, as with impure anthracene, they are due, not to the mass of the material, but to a trace of a coloured substance which is not, like chrysogen, entirely decomposed by sunlight, but only so far modified as to occasion the above changes.

To avoid circumlocution in speaking of these bodies in future, I would propose to call the white material petrolescene, from its source, fluorescence, and general analogy to anthracene and the colouring-matter which is the source of the brilliant fluorescence by which my attention was first drawn to the body thallene, from the two brilliant green lines which are the most prominent characteristics of its spectrum.

I should mention that petrolescene is distinguished from anthracene by its high boiling- and melting-point (about 700° F.), by its very sparing solubility in boiling alcohol and benzine, and by its crystallizing in spirules and not in scales.

Thallene differs from chrysogen in its spectra of fluorescence and absorption, and in its action under the influence of sunlight.

My friend Dr. Geo. F. Barker, to whom I am indebted for references to some original papers and aid in procuring a supply of material, has kindly undertaken the chemical examination of these bodies; and in connexion with him I hope soon to report more fully on the subject.

I wish here also to acknowledge my obligation to Mr. W. E. Geyer, my assistant, and to Messrs. P. P. Poinier and A. H. G. Sorge, students in the Institute, for various assistance in carrying on the observations.

XLII. *On the Nutrition of Muscular and Pulmonary Tissues in Health and when affected with disease from Phthisis. By* WILLIAM MARCET, *M.D., F.R.S.*[*]

PART I. *On the Nutrition of Muscular Tissue in Health.*

THE object of the present memoir is to give a description and an explanatory statement of the investigation I have undertaken into the phenomena of the nutrition of animal tis-

* Communicated by the Author.

sues, these inquiries relating more particularly to the nutrition of muscles and lungs in health and when affected with phthisis : and I must begin by acknowledging the valuable assistance of Messrs. H. Bassett, F. A. Manning, and M. J. Salter in the analytical portion of the inquiry ; I am much indebted to these gentlemen for the care they have bestowed on the work.

The subject is treated by methods of investigation which may be considered new ; it is therefore necessary that I should enter into their details, so as to make the mode of reasoning, the analytical process adopted, and the results obtained equally and thoroughly clear to the reader.   By this means only can I hope to forestall objections and establish the correctness of my work.

I must beg leave to begin with a few introductory remarks relating to liquid diffusion, a subject which has been so admirably treated by Graham.

If we suppose a solution of common salt, on which a flat piece of cork is floated, and if a stream of water be poured carefully upon the cork, the water will not mix immediately with the solution of salt, but form an upper layer in the receiver, while the solution will occupy the inferior layer.   Supposing no cause whatever to agitate the fluids, that they be neither shaken nor subjected to any current of air, they will, however, undergo a tolerably rapid process of mixing, the solution of salt moving into the water, or, in other words, distributing itself throughout the water. This phenomenon is called *Liquid diffusion*.

The rate at which diffusion takes place varies according to the substance in the solution ; hence it is said that different solutions have different rates of diffusibility.   Chloride of sodium may be regarded as yielding aqueous solutions possessed of this property in a very high degree ; while white of egg or blood allow of the distribution of their albumen through water at a very slow rate indeed.

Now, supposing that a jelly be prepared, by dissolving isinglass in a weak solution of chloride of sodium (in a strong solution the jelly may not set).   If distilled water be poured over this jelly, the salt will by degrees find its way out of the jelly into the water, and will continue doing so until it be distributed equally throughout the jelly and the water.   Should the water and the jelly occupy the same bulk, we shall find, after a certain number of hours, the same amount of chloride of sodium in the jelly and the water. Should the volume of the water be twice that of the jelly, a certain bulk of the water, after complete diffusion, will only contain half the amount of salt present in an equal bulk of the jelly, and so on.   On the other hand, if a solution of common salt in water be poured over a jelly of gelatine, after a time the salt will be found distributed throughout the water and jelly propor-

tionally to their respective volumes. Should a jelly be prepared consisting of a mixture of a solution of isinglass and white of egg, it will exhibit, with reference to the albumen it contains, diffusible properties entirely at variance with those observed in the case of the mixture of jelly and salt. When water is poured over this albuminous jelly, the albumen will not diffuse out, or its diffusion will be extremely slow; hence a jelly containing albumen has such a thorough hold upon it that this substance can no longer be extracted from the jelly; no amount of trituration or pounding or washing will separate the albumen; this simple want of diffusibility caused the albumen to become firmly united with, or fixed by, the isinglass jelly.

Graham has observed that, as a rule, substances possessed of the property of crystallizing (such as common salt or sugar) yielded solutions much more diffusible than those of substances which were not possessed of the power of crystallizing, such as gelatine; hence he has classed substances into *crystalloids* and *colloids.*

How can we explain these phenomena, unless it be admitted that there existed a degree of attraction or adhesion between the albumen and the jelly greater than that occurring between the salt and the jelly—so that in the one case the albumen was fixed in the jelly, while in the other the salt moved freely out of it? Substituting the simpler cases of pure white of egg and a solution of common salt in water, the different degrees of diffusibility exhibited in these two instances will admit of a similar explanation, the water retaining the albumen in one case, and letting out the salt in the other. If this view be taken of the cause of the various degrees of diffusibility of different solutions, it must be acknowledged that there exists a certain attraction between substances and the water which holds them in solution; and this attraction varies in its degree according to the substance.

I propose, for want of a better denomination, to call this by the name of *colloid* attraction, and to say that the albumen in white of egg is held to the water by "colloid attraction." I therefore retain the names *colloid* and *crystalloid* given by Graham—colloids not being possessed of the power of crystallizing, and being sparingly diffusible, while crystalloids are crystallizable substances, yielding readily diffusible solutions.

Crystalloid solutions never gelatinize; colloid solutions either gelatinize or solidify into a thick, gummy, adhesive substance, which dries into a residue exhibiting, frequently, somewhat the appearance of a varnish.

This colloid attraction, which keeps water and isinglass united together in a jelly, is also apparently concerned in the formation and physical existence of animal tissues. Muscular

tissue is formed of fibres running parallel with each other in the form of bundles, which are not in mutual contact, but separated from those in their immediate vicinity by connective tissue. These fibres consist physically of animal matter and water, held together by a peculiar power which cannot be considered due to a chemical property, but appears to exhibit the character of colloid attraction. The present view rests on the following considerations :—

1st. That muscles have a soft pliable consistence, and are dry to the touch as a jelly would be.

2nd. That Kühne, of Heidelberg, has obtained from muscular tissue a real jelly he has called *myosine*.

3rd. That muscular tissue contains a proportion of water which does not appear to vary in health.

4th. That chloride of sodium, in a certain proportion, interferes with the setting of gelatine; and muscular tissue is nearly free from this substance, while blood (which remains liquid) contains it in a comparatively large quantity. And it is worth noticing that when blood loses its chloride of sodium by dialysis (diffusion) it becomes considerably thickened.

5th. That after removing by diffusion (dialysis) certain diffusible substances which muscles contain in the small proportion of about 25 per 1000, there remains a mass differing, it is true, from a jelly, inasmuch as it yields a solution of colloid substances by trituration in water, but like a jelly in the fact that the removal of these colloid substances leaves a material consisting of substances in a semisolid condition, which are fixed by the water present; no amount of trituration or pounding or squeezing in water will alter the composition of this soft solid mass, which, if it were not for its tenacity and fibrous consistence, would possess in many respects the characters of a jelly, holding certain proportions of albumen and other equally colloid substances.

The fact of there being a fixed proportion of water in muscular tissue is remarkable. The consistence of a jelly depends on the amount of water it contains; a solution of gelatine in too large a bulk of water will not set at all, while the less water this solution contains the more solid the jelly will be. Now it is but fair to assume that muscles must have a certain fixed consistence for the normal performance of their functions; and if their consistence depends on the proportion of water present, as in the case of a jelly, muscles must contain a fixed proportion of water, which they really do.

Kühne has succeeded in extracting from the muscles of frogs immediately after death a substance which sets into a firm coagulum.

" If a frog be opened, a 1-per-cent. solution of chloride of

sodium driven through the blood-vessels until all the blood is removed, the muscles then rapidly chopped up and subjected to firm pressure, a liquid will be obtained which in a short time sets into a firm coagulum."—*Myologische Untersuchungen* (extracted from Watts's Dictionary of Chemistry).

Therefore juice of flesh has a tendency to coagulation, as would a solution of gelatine ; this tendency must be possessed by those substances in juice of flesh which are soluble and colloid, and therefore, as I shall show, destined to the nutrition of flesh, or to become transformed into muscular tissue.

I conclude that there is a strong ground for the belief that the elementary physical constitution of muscle is that of a jelly—with this difference, that it is organized so as to possess due tenacity for the performance of its functions ; but the water, albumen, and other constituents hold apparently the same relation to each other as water would to gelatine in a jelly *.

Bone may be considered as consisting originally of a jelly of a colloid material and water, the water being subsequently replaced by phosphate and carbonate of lime and magnesia, which are united with the colloid material much in the same way as the water had been originally united to this same material. The existence of a colloid constituent of bone very much resembling gelatine is easily demonstrated by the well-known experiment of immersing a bone in dilute hydrochloric acid, when the earthy matters are removed, water taking their place and entering into a colloid union with the gelatinous matrix, the union being apparently similar to that which had existed before between the earthy matters and the colloid material. The connexion between water and gelatine in a jelly obviously takes place between two colloid bodies, although water may under certain circumstances, as under the influence of cold, assume the crystalloid condition ; and moreover we find that, in the formation of bone, phosphate and carbonate of lime and magnesia exist in an amorphous or non-crystalline state ; I therefore consider these earthy substances as existing in a colloid condition in osseous tissue.

Animal tissues, although in some respects resembling a jelly, vary, of course, essentially from this colloid material because of their having a definite structure. Virchow has discovered with the microscope that there exists in muscular and other tissues a complex system of minute channels, the object of which is apparently to allow of the transmission of the nutritive material to the different parts of the tissues. Indeed it is very difficult, not to say impossible, to account for the distribution of the colloid material destined to nourish tissues after it has left the blood,

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* Nerves and vessels form such a very minute proportion of muscle that I have overlooked them in the present inquiry.

unless the presence of these channels be admitted. That such a nutritive material really exists must be acknowledged, as (Quain's 'Elements of Anatomy,' vol. ii.) "the capillaries destined for the proper tissue of the muscle form among the fibres a fine network with narrow oblong meshes, which are stretched out in the direction of the fibres . . . . none of the capillary vessels enter the sarcolemma or proper sheath of the fibre." There must consequently be a material intermediate between blood and tissue, reaching every particle of the tissue to be nourished ; and with this object in view, there must exist proper means for the thorough distribution of this material.   I have shown (*Bibl. Universelle*, Feb. 1865), by a very simple observation of a physical nature, and without the use of a microscope, that a system of channels ramifies through muscular tissue, containing the material destined to the nutrition of flesh.

On considering the physical condition of flesh, it occurred to me that there would be no difficulty in determining whether muscular tissue is strictly a colloid mass like a jelly or not, by merely immersing a piece of muscle or raw meat in water. Should it be a solid colloid body, no albumen could be expected to diffuse out of the meat into the water; on the other hand, if it was a porous mass, and should these pores or minute channels contain albumen, some of the substance would necessarily pass out of the meat into the water by a process of porous distribution, as would take place if a sponge containing white of egg were immersed or hung up in water.

It is an observation nearly of daily occurrence that raw meat steeped in cold water yields albumen. 200 grammes of ox-flesh were minced and extracted with 125 cub. centims. of distilled water, the phosphoric acid and albumen being subsequently determined in the extract.   On the other hand, a piece of raw beef weighing 200 grammes was immersed for 26 hours in 125 cub. centims. of distilled water, when the phosphoric acid and albumen were also determined in the fluids : the result of the analysis showed that the amount of albumen which had passed out of the meat in the water was less than half of that which had been obtained by extraction ; while there was separated by diffusion more than half the proportion of phosphoric acid contained in the extract. The numerical results were as follows :—

	In 100 cub. centims. extract.	In 100 cub. centims. fluid in which the flesh had been immersed.
Phosphoric Acid	0·233	0·169
Albumen	2·925	1·067

This experiment shows that flesh is permeated in every direction throughout its mass with a multitude of minute channels charged

with the material destined to its nutrition, to which the albumen belonged. It might be objected that a small quantity of blood was possibly left in the tissue after slaughtering, which would account for the presence of albumen in the water in which the meat was steeped; but meat from slaughtered animals is perfectly free from blood. On triturating minced ox-flesh with salt water I could not find any blood-corpuscles by subjecting various portions of the mass to microscopical examination, while on adding one or two drops of serum containing some blood-corpuscles to a few ounces of the pulpy mass, and agitating the whole together, the blood-corpuscles could be detected most readily. In the tissue of the heart of the ox, however, I usually found small quantities of blood, and had to give up determining the albumen in extracts of that organ because of the results being too high on that account. On these occasions I had no difficulty in detecting the presence of blood.

Returning to my subject, I hope to have succeeded in showing that muscular tissue consists of a solid material permeated by channels containing an albuminous fluid, and that the constituents of the solid material are bound together by a force similar to that which connects gelatine and water in a jelly.

On the Mode of Nutrition of Tissues.

A tissue consists of a solid portion containing a fluid nutritive material within its mass. It must appear obvious at the outset that if the solid portion is colloid, the material for its formation must also be colloid; indeed it is well known that albumen, a thoroughly colloid substance, takes a considerable share in the process of nutrition. I shall show that the phosphoric acid, together with the small quantity of potash (and, we may assume, also the magnesia), which enter into the composition of the nutritive material are also colloid; muscular tissue contains, however, nearly 25 per 1000 of *crystalloid* material, consisting of potash and magnesia salts, and very small proportions of chlorine and soda, together with crystalloid organic nitrogenized substances, such as kreatine and kreatinine. It occurred to me that the formation of these crystalloid substances was due to the process of waste—a view which derived some support (before it was thoroughly investigated) from the fact that the urinary secretion consists of diffusible substances; the transformation of colloids into diffusible crystalloids appeared moreover at the outset a convenient method for a process of elimination; and also, blood being much more colloid than tissues, could hardly be considered the source of the crystalloid substances they contain.

The nitrogenized crystalloids in tissues would result entirely, according to this view, from a transformation of assimilated albu-

men with a view to its ultimate elimination. The investigation upon which I now beg to enter, extending over a period of about five years, proved the correctness of this theory. A tissue is constantly undergoing change. Very soon after it attains its highest stage of development, or its state of maturity, it dies, and is decomposed into crystalloids. Dr. Beale ('Life Theories' &c.) believes that as soon as what he calls the bioplasm is transformed into the insoluble matrix of a cell, it dies, then disappears, and is replaced by other cells*. We may therefore regard tissues as formed of three different materials:—(1) the nutritive material which has left the blood and is on its way to become assimilated; (2) the fully developed or ripe tissue; (3) the material resulting from the waste of tissues, which is on its way out as effete matter.

After much time and consideration had been devoted to the available means of separating from each other these three different materials and effecting their analysis, I adopted the following process, which answered the purpose most satisfactorily.

If, say, 200 grammes of flesh be minced thoroughly and mixed with 500 cub. centims. of water into a homogeneous pulpy mass, there will be obtained, after straining through calico or muslin, about 500 cub. centims. of extract (including that wetting the calico), while about 154 cub. centims. of solution will remain in the fibrous mass left in the calico. This solution is estimated by drying the weighed fibrous mass, the loss of weight so obtained representing the volume of the extract without any material error. The total extract will therefore be equal to 654 cub. centims., and will contain:

1st. The whole of the colloid material on its way to form flesh;

2nd. The whole of the crystalloid material resulting from the waste of the tissue and on its way out of flesh;

3rd. Probably a small portion of colloid material in progress of assimilation, which is squeezed out by the process of extraction.

The fibrous portion in the muslin, imagined dry and free from extract, will represent a mass weighing rather less than 46 grms., and consisting of colloid material assimilated and insoluble in water plus a small portion of colloid material in process of assi-

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* Beale states:—" Every tissue may be divided anatomically into *elementary parts* [sic]. Each elementary part consists of the *living matter* or *bioplasm* and the *lifeless formed matter* (cell-wall, envelope, tissue, intercellular substance, periplastic matter) produced at the moment of the death of the particles of the first." Beale, therefore, apparently considers as dead organized particles what I have called *ripe or mature tissue*, which, however, is on the point of becoming dead and lifeless. It appears to me *mature* because it is in this state only that it can perform its functions.

milation. This partly assimilated colloid material has the same composition as that of the insoluble fibres; indeed I shall be able to show that the whole of the colloid material destined to become assimilated has the same composition as the fully developed and insoluble tissue; so that the passage from fluid to solid is a mere morphological change.

I must now state how the composition of the above three different classes of materials was determined, these materials constituting :—

1st. The fibrous insoluble mass;

2nd. The colloid fluid, destined to form the insoluble mass;

3rd. The crystalloid solution, destined to remove from flesh the effete material it contains.

A few words will suffice, I trust, to make the method of analysis quite clear. Having prepared the extract of 200 grammes of flesh with 500 cub. centims. of water as stated above, the albumen, phosphoric acid, and potash, with the soda and chlorine, it contained were determined. On the other hand, the fibrous mass in the muslin, after the estimation of its water, was submitted to analysis for the determination of the same substances (with the exception of chlorine), and also of its nitrogen, which was done by combustion with soda-lime. The water (estimated by desiccation) represented the bulk of the extract left after straining the fibrous mass; and the composition of this portion of the extract (retained in the fibres) was calculated from that which had been found for the solution separated from the flesh by extraction. Now, by subtracting respectively the numbers obtained for the constituents of the portion of the extract retained in the fibrous mass from those found for the constituents of this fibrous residue, it is obvious that the result represented the composition of fibrous mass free from extract. The composition of the colloid material in solution destined to nourish the tissue was calculated from that of the insoluble fibrous mass (insoluble fibrous mass considered free from extract), assuming that the relation the constituents held to each other in both cases was the same—an assumption which I shall show to be correct. The proportion of albumen assimilated, calculated from the nitrogen found in the insoluble fibres, and of soluble albumen in the total colloid solution, were taken as starting-points for the calculation.

Finally, the composition of the crystalloid material was calculated by adding together the numbers representing the proportions of the constituents of the insoluble fibres and nutritive colloid solution, and subtracting the result from the *total* quantity of each constituent respectively found in 200 grammes of flesh.

A simple way of explaining this will be as follows. Let the

albumen, phosphoric acid, and potash be represented respectively :—in the insoluble fibrous mass by

$$A, \quad B, \quad C \, ;$$

in the soluble colloid material by

$$A', \quad B', \quad C' \, ;$$

in the total flesh by $A'', \quad B'', \quad C'' \, ;$

in the crystalloid material by

$$A''', \quad B''', \quad C'''.$$

B' and C' are calculated as follows :—

$$A : B = A' : B',$$
$$A : C = A' : C',$$
$$B' = \frac{B + A'}{A},$$
$$C' = \frac{C + A'}{A}.$$

The crystalloid material has also to be calculated,

$$A''' \text{ being } = A'' - (A + A'),$$
$$B''' \quad\quad = B'' - (B + B'),$$
$$C''' \quad\quad = C'' - (C + C').$$

The colloid material was calculated, as previously stated, on the assumption that it possessed the same relative composition as the insoluble fibrous mass; and I must now show the truth of this theory. It will be necessary to begin by establishing the fact of the colloid nature of the solution. The proof that this material is colloid is derived from the consideration that albumen, its main constituent, is possessed of strictly colloid properties. Moreover, juice of flesh contains, as I have observed by submitting it to dialysis, a certain quantity of phosphoric acid and potash which, even after 24 hours does not pass through the diaphragm of the dialyzer, and may therefore be considered as colloid. With reference to the composition of the colloid solution, it is difficult to obtain accurate quantitative determinations of colloid phosphoric acid and colloid potash by dialysis; still, by submitting to this process for 24 hours minced flesh made into a pulp with water, the proportions of albumen, colloid phosphoric acid, and colloid potash were found in the following analysis very much the same as those obtained for the

albumen, phosphoric acid, and potash in the insoluble fibrous portion of the tissue. The numerical results were :—

Total colloid constituents of 200 grammes of ox-flesh prepared by dialysis.			Mean composition of the mature tissue calculated in 5·74 albumen.
		Proportion calc. in 5·74 albumen.	
Albumen (determined as nitrogen)	38·06		
Phosphoric acid......................	0·375	0·056	0·051
Potash	0·132	0·020	0·017

In this analysis the colloid substances contained in solution in juice of flesh plus these same substances as constituents of the insoluble fibrous mass, bore to each other respectively very nearly the same relation as the constituents of the mature or insoluble tissue. Other experiments undertaken by dialyzing *extracts* of muscular tissue yielded similar results ; it happened, however, occasionally that they differed—obviously on account of the method of analysis, by means of a dialyzer not being invariably reliable.

It follows that the material destined to become transformed into the insoluble portion of flesh, or ripe tissue, undergoes a mere morphological change, one molecule of the ripe tissue as it wastes away being replaced by one molecule of the nutritive material having precisely the same composition.

I shall not take into account my earliest analyses, amounting to fourteen in number, which are incomplete in several respects. Moreover the methods of analysis then adopted were not near so correct as those with which the subsequent inquiry was conducted. I have to report seven analyses of muscular tissue. For the first four, the flesh was taken from the muscles of the neck of as many oxen immediately or but very few hours after slaughtering. For the last three analyses, the muscular tissue was obtained at the Consumption Hospital (Brompton) from human subjects after death from phthisis, before decomposition had set in. The composition of these last three samples varied but slightly from that of the other four ; and the result respecting the phosphoric acid and potash effete was precisely the same. I shall at present consider only the first four analyses.

Table showing the Constituents of Muscular Tissue in 200 grms.

Class No. 1.

Composition of the material forming the mature and insoluble Tissue.

	I.		II.		III.		IV.		Mean.
	In 200 grms. ox-flesh.	Propor-tion	In 200 grms. ox-flesh.	Propor-tion.	In 200 grms. ox-flesh.	Propor-tion.	In 200 grms. ox-flesh.	Propor-tion.	In 200 grms. ox-flesh.
Albuminous material	31·16	100	27·216	100	26·6	100	27·3	100	28·070
Phosphoric acid	0·189	0·606	0·239	0·88	0·3	1·13	0·276	1·01	0·251
Potash	0·054	0·173	0·017	0·06	0·244	0·94	0·03	0·11	0·086
Magnesia	......	......	......	......	0·423	1·6	0·509	1·86	

Class No. 2.

Composition of the material destined to become Flesh, entirely Colloid.

	In 200 grms. ox-flesh.	Propor-tion	In 200 grms. ox-flesh.	Propor-tion.	In 200 grms. ox-flesh.	Propor-tion.	In 200 grms. ox-flesh.	Propor-tion.	In 200 grms. ox-flesh.
Albuminous material	6·62	100	5·67	100	5·265	100	5·428	100	5·745
Phosphoric acid	0·04	0·604	0·050	0·882	0·060	1·13	0·055	1·01	0·050
Potash	0·011	0·166	0·003	0·053	0·048	0·91	0·006	0·11	0·017
Magnesia	......	......	......	......	0·009	1·7	0·049	0·18	

Class No. 3.

Composition of the effete material on its way out of Flesh, entirely Crystalloid.

	In 200 grms. ox-flesh.	Propor-tion	In 200 grms. ox-flesh.	Propor-tion.	In 200 grms. ox-flesh.	Propor-tion.	In 200 grms. ox-flesh.	Propor-tion.	In 200 grms. ox-flesh.
Albuminous material	3·64	100	3·622	100	3·622	100	3·913	100	3·70
Phosphoric acid	0·595	16·34	0·62	17·12	0·518	14·3	0·521	13·31	0·563
Potash	0·762	20·93	0·803	22·17	0·723	19·96	0·759	19·69	0·764
Magnesia	......	......	......	......	0·025	0·69	0·0955	2·44	

	Found.				Mean found.	In 100 $2KO\,PO^5$. Theory.
Phosphoric acid	43·8	43·7	41·7	40·4	42·4	43·0
Potash	56·2	56·3	58·3	59·6	57·6	57·0

I shall now beg to offer a few remarks on the mature fibrous and insoluble material, and on the effete crystalloid constituents of flesh.

I. *The insoluble fibrous material.*

It is this part of muscular tissue which may be considered as giving muscles their tenacity and contracting power.   It is

formed of molecules disposed according to a certain definite structure, and consisting of albumen, phosphoric acid, potash, and magnesia, which, however, do not exhibit fixed proportions, but vary within certain limits.

These materials are constantly undergoing destruction, and may be considered as dying very shortly after they are ripe.

The regularity with which the nutrition of this mature portion of flesh takes place is strikingly shown by the fact that the soluble and coagulable albumen in a given weight of tissue always bears the same proportion to that of the albumen assimilated in the insoluble fibrous mass. The absolute quantity of albumen assimilated in a given weight of muscle varies, probably in a great measure on account of the different proportions of fat muscle contains; but the relation of this assimilated albumen to the soluble and coagulable albumen remains very nearly the same in every case, as shown in the following Table :—

Found in 200 grammes of ox-flesh.		In 100.	
Albumen insoluble.	Albumen.	Albumen insoluble.	Albumen.
Analysis I. 31·16	6·62	82·48	17·51
„ II. 27·216............	5·67	82·76	17·24
„ III. 26·6	5·265	83·48	16·52
„ IV. 27·3	5·428	83·40	16·60
Mean...................	....	83·04	16·97

From which results the fact, that for *every molecule of albumen assimilated or converted into insoluble muscular tissue, an equal quantity of albumen is withdrawn from blood into the tissue.* This mean proportion of soluble to insoluble (or assimilated) albumen is as 16·97 to 83·14, or 1 to 4·9, which means that there is 4·9, say 5 times as much assimilated albumen in flesh as in its nutritive fluid.

II. *The effete material on its way out of flesh entirely crystalloid.*

This includes perhaps the most interesting results from my inquiries.

I shall first beg to draw attention to what I have called *the effete albuminous material,* and compare it with the corresponding constituent of the fluid destined to nourish flesh. What I have considered as the albuminous material of the third class is crystalloid, having assumed the form of kreatine, kreatinine, &c. It was determined by evaporating to dryness the filtrate and washings from the coagulated albumen of a known bulk of the extract. A small quantity of sulphate of lime was

added to the fluid during the evaporation, so that there was no difficulty in taking up the residue for the combustion with soda-lime; 15·7 parts of nitrogen corresponded to 100 of albumen. Notwithstanding the variety of substances into which albumen is thus transformed, we find a relation between the albuminous materials of both classes which does not vary between wide limits. Thus:—

	Albumen coagulable and colloid, 2nd class.	Material from albumen crystalloid, 3rd class.	Relation.
	6·62	3·64	1·82 : 1
	5·67	3·622	1·57 : 1
	5·265	3·622	1·45 : 1
	5·428	3·913	1·30 : 1
Mean...	5·746	3·699	1·56 : 1

Hence the nutritive fluid of flesh contains a mean of rather over one half more albumen than is present in the solution of the effete material. Now it is obvious that if a muscle should retain a certain composition, which it does within certain limits, it must draw upon the blood in proportion to its waste. Therefore for every 3·699 grammes of albumen (in the crystalloid form) on its way out, 200 grammes of flesh must draw 3·699 grammes of albumen colloid and coagulable from the blood. But we find 200 grammes of muscular tissue to contain a mean of 5·746 grammes of colloid coagulable albumen; and as the albumen must regulate the supply of the other substances muscular tissue requires for its nutrition, it follows that about one third of the whole of the nutritive material present in flesh is in store, not being required for immediate use. Therefore if the blood, from want of food, were incapable of nourishing flesh, yet the muscle would apparently continue for a certain time deriving food from the material accumulated within the tissue. This appears to be a provision of nature to allow of muscular exercise during prolonged fasting. Of course this view must be considered a mere deduction open to future investigation.

The relation of phosphoric acid and potash to albumen in the third class varies in the different analyses, the former between 13·31 and 17·12 per cent. (of the albumen), and the latter between 19·80 and 22·17 per cent.; but it is highly interesting to observe that these two substances, relatively to each other, are present in muscle as effete material precisely in the proportion of 43 of phosphoric acid to 57 of potash, correspond-

ing to pyrophosphate of potash, which may be originally the neutral tribasic phosphate of potash subsequently decomposed by incineration. It will be observed that these four analyses all yield a similar result, which will be found confirmed by the composition of the human muscles subjected to analysis*. This result is the discovery of the existence of phosphoric acid and potash in the effete material exactly in the right proportion for the formation of *pyrophosphate of potash.*

I believe this is the first time the composition of an inorganic chemical compound has thus been determined by bringing together, theoretically, its constituents in an animal tissue. The formation of this substance is very remarkable; it shows beyond a doubt that blood yields (besides albumen, phosphoric acid, and small quantities of potash and magnesia to be transformed into flesh) a large proportion of potash the only object of which is the removal of the phosphoric acid of the ripe muscular tissue. Potash may also be concerned in the oxidation of the albuminous portion of the tissue into crystalloid compounds; and I may remind the reader that Dr. E. A. Parkes has shown that potash taken into the body favours oxidation, causing an increased elimination of urea and sulphuric acid.

The proportions of the constituents of flesh I have introduced under class No. 3 are originally derived from the composition of the extract, from which are subtracted respectively the proportions of the colloid substances present. These colloid constituents are calculated from the composition of the insoluble fibrous portion of the tissue and the proportion of the soluble albumen; so that any error in any one of the determinations of albumen, phosphoric acid, or potash would suffice to vitiate the whole of the result.

Absence of Soda and Chlorine from ripe Muscular Tissue, or the insoluble portion of flesh.

The proportions of chlorine and soda contained in juice of flesh are very small; in eleven analyses of the extracts from the flesh of as many different animals, the proportions of chlorine in 200 grammes varied from 0·094 to 0·212. These results may be considered correct, notwithstanding their being represented

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* The proportions of phosphoric acid and potash effete in human muscular tissue after death from phthisis were as follows in three analyses :—

	Analysis I.	II.	III.	Mean.
Phosphoric acid	43·2	42·9	42·7	42·9
Potash	56·8	57·1	57·3	57·1

by such low numbers. They were obtained by the dialysis of a certain bulk of the extract, and determined volumetrically in portions of the fluid outside the dialyzer, being finally calculated for the whole bulk of the fluid in and out of the dialyzer.

The proportion of soda present varied in six analyses from 0·155 to 0·333 gramme in 200 grammes of flesh submitted to analysis, being about twice as much as the chlorine would require to be made into chloride of sodium. Some of the soda is therefore eliminated in combination with one or more of the organic acids resulting from the decomposition of the organic portion of flesh.

My present object, however, is mainly to show that chlorine and soda take no part in the actual formation of flesh. With this object in view, 300 grammes of sheep's flesh were minced and extracted with 750 cub. centims. of water, as usual. The fibrous mass and dry extract were incinerated slowly with pure lime, and the ash was mixed with water, in which the chlorine was determined volumetrically.

The extract retained in the fibrous mass yielded (by calculation) 0·032 gramme of chlorine, and the fibrous portion 0·035 gramme of chlorine—the difference amounting to 0·003, or 0·001 gramme per 100 grammes of flesh, which is insignificant and proves the absence of chlorine in the ripe or insoluble tissue.

The experiment relating to the *soda* was undertaken by mincing 200 grammes of ox-flesh, adding water to the mass, and dialyzing the whole for twenty-four hours. The soda was then determined in the diffusate; and the amount of diffusible soda retained in the colloid portion was calculated from the volumes of the fluids in and out of the dialyzer. This was subtracted from the soda found in the total colloid mass—the difference amounting to only 0·004 gramme, or 0·002 gramme per 100 grammes of flesh, which is insignificant; and I conclude that the ripe or insoluble muscular tissue contains no soda. The object of the presence of chloride of sodium in flesh appears to me to be connected with the distribution of water throughout the tissue. This would be a subject interesting to investigate, and likely to yield important results.

On the Constitution and Nutrition of the Muscular Tissue of Fish.

My inquiry on the nutrition of the muscular tissue of fish is limited to an analysis of salmon's flesh, which has yielded the following results :—

Composition of Salmon's Flesh, in 200 grms.

	Composition of insoluble tissue.	Composition of nutritive material.	Composition of effete material.
Albumen	25·16	12·470	4·360
Phosphoric acid...	0·171	0·085	0·945
Potash	0·065	0·032	0·828
Soda, total found	......	......	0·058

	Effete, in 100 parts.	Theory.
Phosphoric acid . .	53·3	43
Potash	46·6	57

In the present case the proportion of albumen of the nutritive material is no less than twice as large as in ox-flesh; and about two thirds of the amount of this substance present was in excess of that required for immediate use. The necessity of this large store of nutritive material in salmon's flesh may be accounted for by a consideration of the rapid growth of the fish, amounting in a few months to several pounds during their migration to the sea. (I could not ascertain where the fish was taken.)

The high proportion of phosphoric acid and potash in the effete state is a remarkable circumstance, considering that salmon is constantly subjected to loss of substance from liquid diffusion; but this is explained by the fact that phosphoric acid and potash in salmon, in the effete condition, are not present in the exact proportion to make a crystalloid potash salt; there is an excess of phosphoric acid present; and therefore these substances are less crystalloid and consequently less diffusible than in the higher class of animals.

[To be continued.]

XLIII. *On the Connexion of the Second Proposition of the Mechanical Theory of Heat with* Hamilton's *Principle. By* R. Clausius*[*].

INDUCED by a memoir of mine published not long previously[†], M. Szily has instituted an interesting consideration of the second proposition of the mechanical theory of heat[‡], in which he arrives at this result—that the equation which expresses that proposition can be deduced as an immediate consequence from Hamilton's principle.

That the second proposition of the mechanical theory of heat is connected with the principle of least action was also stated by me, and, as I subsequently learned, also still earlier by Boltzmann[*]. This connexion becomes, as M. Szily quite correctly insists, still more striking when the amplified expression given by Hamilton of the principle of least action is employed.

But for this purpose Hamilton's equation must not be taken in the form in which it is usually cited in the text-books of mechanics and is found, for example, in Jacobi's 'Lectures on Dynamics,' p. 58 : viz., for a system of material points in motion under the influence of forces which have a force-function or ergal, let the *vis viva* be denoted by T and the ergal by U, in the sense that the sum T + U is constant; then the usually cited form of Hamilton's equation is

$$\delta \int (T - U) \, dt = 0. \quad \ldots \quad \ldots \quad (1)$$

In order that this equation may be correct, the variation signified by δ must be so understood that in variating we neglect the alteration of the time—although in reality the altered motion to which the variation refers is different from the original motion not merely in respect of the coordinates and velocities, but also of the time in which it happens.

If, on the contrary, we understand the variation-symbol δ in the sense usual in other cases, as signifying the entire alteration of the variated quantity, the equation must read :—

$$\delta \int (T - U) \, dt + (T + U) \delta \int dt = 0,$$

or, if we denote by i the duration of time to which the integration refers, and for the sum T + U (the *energy* of the system) introduce the symbol E,

$$\delta \int_0^i (T - U) \, dt + E \delta i = 0.$$

The same equation can, as is readily seen, be also brought into the following still more simple form :—

$$2\delta \int_0^i T \, dt = i \delta E. \quad \ldots \quad \ldots \quad \ldots \quad (2)$$

This is the form of Hamilton's equation made use of by Szily, and which expresses the principle of least action with variable energy. It agrees with Boltzmann's equation (23 a) in the above-cited memoir, if we understand under the quantity which he denotes by ϵ not merely the *vis viva* supplied, but, in accord-

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[*] *Sitzungsberichte der Wiener Akademie*, vol. liii.

ance with his later explanation, the augmentation of *vis viva* and work, so as to attribute to it the same signification as to the variation δE in equation (2) above.

Szily derives, in the simplest manner possible, from equation (2) another equation, which he regards as synonymous with the second proposition of the mechanical theory of heat. In relation to this I cannot help saying that his development appears to me too simple, because in it important difficulties remain unnoticed and unsolved.

Of that mechanical equation which I produced and employed in my memoir mentioned at the commencement he speaks as if it were contained in Hamilton's equation. But that is not possible; for my equation possesses a more general applicability than Hamilton's. The latter, namely, presupposes as a necessary condition that with the altered motion the ergal is to be expressed by the same function of the space-coordinates as with the original motion; while my equation remains valid even when the function of coordinates which represents the ergal undergoes an alteration. As the simplest case of this kind, we may assume that the function contains, besides the coordinates, also a quantity which with each motion remains constant. Hamilton's then presupposes that this quantity with the altered motion has the same value as with the original motion, while mine permits an alteration of the value of this quantity.

I have already, in a memoir * relative to Boltzmann's, explained this difference between the two equations, and have there shown more particularly how far Hamilton's equation would be incorrect for the case in which the ergal undergoes an alteration independent of the alteration of the coordinates.

The erroneous supposition that my equation can be derived from Hamilton's appears to me to have arisen from this, that M. Szily has not quite correctly understood my notation.

For the more convenient elucidation of the matter, we will here confine ourselves to the consideration of a single moveable point. Given, then, a material point of mass m, which with the original motion describes a closed path or moves between two given points. Let it likewise with the altered motion describe a closed path or move between two points, which latter may either be identical with those previously given, or, when that is not the case, fulfil the condition that the quantity

$$\frac{dx}{dt}\,\delta x + \frac{dy}{dt}\,\delta y + \frac{dz}{dt}\,\delta z$$

has the same value at the final point of the motion as at the

* Pogg. *Ann.* vol. cxliv. p. 268.

initial point*. If we then retain the letters i and E for the duration of the motion and the energy respectively, and denote by v the velocity of the point, and if we further, with variable quantities, indicate their mean value by putting a horizontal stroke over them, we can give to Hamilton's equation as quoted under (2) the following form :—

$$\delta(m\overline{v^2}i) = i\delta\mathrm{E}.. \quad . \quad . \quad . \quad . \quad . \quad (2\,a)$$

In first forming my equation† I wrote it thus :—

$$\delta\overline{\mathrm{U}} = \frac{m}{2}\,\delta\overline{v^2} + m\overline{v^2}\delta\log i. \quad . \quad . \quad . \quad (3)$$

But I did not there employ the letter U as a universally valid symbol for the ergal, but said explicitly that it only denoted the ergal *for the original motion.* In a subsequent memoir‡, in order to show the difference very clearly, I wrote the equation in the following form :—

$$\overline{\frac{d\,\mathrm{U}}{dx}\,\delta x + \frac{d\,\mathrm{U}}{dy}\,\delta y + \frac{d\,\mathrm{U}}{dz}\,\delta z} = \frac{m}{2}\,\delta\overline{v^2} + m\overline{v^2}\delta\log i\,;\ . \quad (3\,a)$$

and I added, " The sum

$$\frac{d\,\mathrm{U}}{dx}\,\delta x + \frac{d\,\mathrm{U}}{dy}\,\delta y + \frac{d\,\mathrm{U}}{dz}\,\delta z$$

cannot at once be regarded as the variation of the ergal, and hence, if the signification of U be extended so that it shall represent the ergal not only for the original, but also for the altered motion, cannot at once be denoted by δU."

If we assume for example the above-mentioned case, that the function U contains, besides the coordinates, also a quantity c which is constant with each motion, but may change its value from one motion to another, then the above sum does not contain that alteration of the ergal which is occasioned by the alte-

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* If the latter condition be not fulfilled, the difference

$$\left(\frac{dx}{dt}\delta x + \frac{dy}{dt}\delta y + \frac{dz}{dt}\delta z\right)_1 - \left(\frac{dx}{dt}\delta x + \frac{dy}{dt}\delta y + \frac{dz}{dt}\delta z\right)_0$$

(the indices 0 and 1 signifying the initial and final values of the quantity) must occur in the following equations. For our considerations, however, the simpler form of the equations suffices, which corresponds to the assumption that the difference is $=0$.

† *Sitzungsber. der Niederrhein. Ges. für Natur- und Heilkunde,* 1870, p. 174 ; and Phil. Mag. September 1871, p. 167.

‡ " On the Application of a Mechanical Equation advanced by me to the Motion of a Material Point round a fixed Centre of Attraction, and of two Material Points about each other," Phil. Mag. November 1871, p. 321; *Nachrichten der Göttinger Gesellschaft der Wissenschaften,* 1871, p. 248 ; *Math. Ann. von* Clebsch *u.* Neumann, vol. iv. p. 232.

ration of *c*. We can therefore in this case write instead of (3 *a*) :—

$$\delta\overline{U} - \frac{\overline{dU}}{dc}\,\delta c = \frac{m}{2}\,\delta\overline{v^2} + m\overline{v^2}\delta\log i, \quad . \quad . \quad (3\,b)$$

by which the characteristic peculiarity of the quantity which stands on the left-hand side of my equation comes out still more distinctly.

In order that we may conveniently compare together the three mutually related equations with which we have to do in the newer considerations on the mechanical theory of heat, in reference to their applicability, it will probably be advisable to summarily recapitulate the differences which prevail between them. The equation which expresses the principle of least action in its original form, and which in our notation is

$$\delta(m\overline{v^2}i) = 0, \quad . \quad . \quad . \quad . \quad . \quad (4)$$

presupposes that the ergal is represented by an invariable function of the coordinates, and also that the energy has an invariable value. In the Hamilton's equation (2 *a*) an invariable function is likewise presupposed, but the energy may vary. Lastly, in my equation both an alteration of the energy and also of the function representing the ergal are admissible.

The latter generalization was absolutely necessary for the application to the theory of heat, because, with the changes of state of a body which there come into consideration, occur variations of the effective forces which are independent of the space-coordinates and cannot be represented by an ergal of invariable form.

Besides this, other difficulties are met with in the cases to be considered in the science of heat, which render the immediate application of the Hamilton's equation (2) inadmissible.

Equation (2) presupposes that all the material points in the system under consideration take for their motion one common time *i*, which with the change of motion changes in like manner for all the points. But if we conceive a body as a system of a great many moving material points, and make even the simplified assumption that all the points move in closed paths, yet we are not at liberty to presuppose that they all describe their paths in the same time, and that with a change of state of the body all the times of revolution change in like manner. Consequently, to take into account this circumstance, special considerations are necessary.

The difficulties become still greater when we drop the assumption that all the points move in closed paths, and admit that their motions are quite irregular. In M. Szily's analysis, however, not one of these difficulties is mentioned.

Bonn, May 1872.

XLIV. *On some Points in the Chemistry of Acid-manufacture.* By H. A. SMITH, *Junior Assistant in the Laboratory of Owens College, Manchester**.

SECTION I. *On the presence of Arsenic in Alkali-manufacture.*

*P*RESENCE *of Arsenic.*—The great drawback in the manufacture of sulphuric acid from pyrites is most undoubtedly the presence of arsenic. Its removal, even if it can be done completely, is a work of difficulty and expense, as in our methods for purification we must take into account the various uses to which the acid is to be put. It is my intention in the present part of my paper to trace this impurity (arsenic) from the original pyrites, through the various operations with which the acid made from it is connected, to the last stages of alkali-manufacture; and to show also that, not content with throwing the injurious gases of sulphurous and hydrochloric acids into the air, alkali-works must bear the blame of polluting the atmosphere with the still more dangerous substance—arsenious acid. Although the amount escaping from a single work is comparatively small, yet, when we consider the number of works using pyrites in the formation of sulphuric acid, we must confess that in the end it mounts up to something very considerable.

General amounts in various pyrites.—There are two things to be looked to in choosing an ore for sulphuric acid-manufacture. 1st. Its breaking property, if I might so call it,—that is, its power of breaking into small lumps without leaving what are technically called "smalls." 2ndly. Its freedom from arsenic.

If we compare the amount of arsenic in published analyses of various kinds of pyrites, we shall be astonished at the difference between these and the amounts of arsenic found in the acid manufactured from the same ores. From ores containing from 0·21 to 0·31 per cent. arsenic we have acid containing from 1 to 1·5 per cent. arsenic, showing that some mistake has been made in the analyses.

Taking, for instance, the analyses of various pyrites given in Richardson and Watts's 'Chemical Technology,' I find that the largest mean percentage of arsenic present varies from 0·31 to 0·33 per cent. arsenic, whilst some are mentioned as containing merely a "trace," and others as being perfectly pure. These results, however, are never corroborated when these ores are being worked on a manufacturing scale.

Being obliged, in my capacity as chemist in an alkali-work, to turn my attention to this subject, I determined to make an extended series of analyses of such specimens of pyrites as were

likely to suit our purpose, and accordingly drew up a Table of results of analyses which differed to a very great degree from those generally given. In the following Table I. I give in Part I. the amounts of arsenic in the various ores given in Richardson and Watts's 'Chemical Technology;' and in Part II. my own analyses are given at full length. The differences are very great; still I remain satisfied of the truth of my own results when I consider that the increase in the different specimens obtained by laboratory analysis was an index to the increase obtained as the result of manufacture.

TABLE I.

PART I.	
Name of pyrites.	Arsenic per cent. Mean.
Spanish	0·21 to 0·31
Belgian	trace
Westphalian	trace
Norwegian..........;......................	none
Irish	0·33
Cornish	0·32
Italian	trace
Swedish	trace
Cleveland	———

PART II.	
Arsenic per cent.	Mean.
Spanish :—	
Tharsis'...1·542, 1·620, 1·644, 1·790, 1·526, 1·700, 1·552, 1·661, 1·686, 1·796	1·6517
Mason's...1·744, 1·810, 1·891, 1·770, 1·661, 1·692, 1·711, 1·754, 1·601, 1·719	1·7453
Belgian......1·000, 0·664, 1·621, 0·743, 1·002, 0·624, 0·972, 0·924	0·9437
Westphalian 1·886, 1·794, 1·802, 1·936, 1·899, 1·900, 1·921, 1·889	1·8783
Norwegian:—	
Hard ... 1·440, 1·916, 1·633, 1·621, 1·648, 1·611, 1·692, 1·631	1·6490
Soft... ... 1·794, 1·731, 1·664, 1·632, 1·700, 1·621, 1·816, 1·709	1·7085

Here the Belgian pyrites contains the smallest amount of impurity; but this had the fault, on being broken, of crumbling very much and leaving a large amount of "smalls." The next in order of purity is the hard Norwegian. This was a good breaking and burning ore, firm, compact, and easily raised to a red heat in the kilns; so it was found preferable to burn this ore, even with the increased amount of impurity, rather than use the Belgian containing a small amount, but which carried along with it the great inconvenience of making "smalls."

Deposit in flue leading from kilns to lead chamber.—This flue when cleaned out presented a strange appearance. Its length was about 20 feet, and, with the exception of about 10 feet from the "kiln" end, was thickly coated (and even partially filled) with a shining deposit, which on examination proved to be a mass of sulphur in a perfectly viscous state, containing (as a mean of four analyses) 46·360 per cent. arsenic trioxide.

When lighted it burned with the ordinary blue sulphur-flame, and on inserting a piece of cold porcelain in the vapour, gave a deposit of sulphur along with a considerable amount of arsenious acid. (See Table II.)

Sulphuric Acid.—Passing along the flue into the chamber, I find the sulphuric acid containing a large amount of arsenic. As an average of twelve analyses, 1·051 per cent. arsenious trioxide is the result; so that in passing through the flue above mentioned it must have lost a large proportion of that originally present in the pyrites. (See Table II.) It is from the sulphuric acid itself that the arsenic should be removed, as this acid is the groundwork of the whole following alkali-manufacture.

Deposit on bottom of lead chamber.—On the bottom of the lead chamber and sometimes on the sides, a grey siliceous mass is found, interspersed here and there with clusters of delicate regular crystals, the transparent elongated prisms of arsenic acid. The transformation from the arsenious acid we find in the flue to the arsenic acid of these crystals, has evidently been completed along with the oxidation of the sulphurous acid. In this deposit the percentage of arsenic trioxide varied from 1·811 to 1·9 per cent., the rest consisting of sulphate of lead and silica. (See Table II.)

Hydrochloric Acid.—When the sulphuric acid made from pyrites is mixed with common salt in the reverberatory furnace for the formation of sodium sulphate, the arsenious acid present in the acid is converted into the terchloride of arsenic, and escapes along with the hydrochloric acid to the condensing-towers. This conversion is very nearly complete, as the amount of arsenic present in the sodium sulphate is very small. In the hydrochloric acid a mean of eight analyses gives 0·69 per cent. arsenic trioxide, the amounts varying from 0·589 to 0·911 per cent. (See Table II.)

Sulphate of Soda.—The amount present in this is very small. As mentioned above, the conversion from the teroxide to the terchloride of arsenic is wonderfully complete, the percentage in the sodium sulphate being only 0·029 per cent. (See Table II.)

This still shows us how careful we ought to be in obtaining pure sulphate of soda for medicinal purposes.

Deposit in flue leading from salt-cake furnace to condensing-towers.—This flue, about 20 feet long, leads for the most part in the open air, from the salt-cake furnace to the condensing-towers.

TABLE II.

Substances in which arsenic is present.	Arsenic (trioxide) per cent.															Mean.
Pyrites (hard Norwegian):—																
Before burning	1·440	1·916	1·633	1·621	1·648	1·611	1·692	1·631	......	......	......	......	......	......	......	1·649
After burning	0·562	0·356	0·469	0·454	......	......	......	......	......	......	......	......	......	......	......	0·465
Sulphuric acid	1·112	1·026	1·051	1·190	1·141	0·991	0·861	1·036	1·100	1·420	1·003	1·032	0·800	1·006	0·999	1·051
Deposit in flue leading from pyrites-kilns to lead chamber	46·499	46·431	45·911	46·600	......	......	......	......	......	......	......	......	......	......	......	46·360
Deposit on bottom of lead chamber	1·861	1·900	1·811	1·825	1·890	......	......	......	......	......	......	......	......	......	......	1·857
Hydrochloric acid	0·711	0·642	0·686	0·599	0·700	0·693	0·589	0·911	......	......	......	......	......	......	......	0·691
Sulphate of soda	0·006	0·044	0·050	0·032	0·024	0·020	0·023	0·021	0·033	0·015	0·009	......	0·060	0·081	......	0·029
Soda-waste	0·611	0·222	0·190	0·462	0·527	0·643	......	......	......	......	......	......	......	......	......	0·442
Carbonate of soda	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—
Recovered sulphur:—																
Mond's process	0·901	0·842	0·616	0·442	......	......	......	......	......	......	......	......	......	......	......	0·700
After purification	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—	—

The part from which the deposit was taken was about 15 feet from the furnace. At first sight the deposit seemed to be composed merely of common salt and a little sodium sulphate; but on examination with a hand-microscope, small octahedral crystals of $As^2 O^3$ were distinctly seen.

TABLE III.

Tons.		Ton As.
100	Hard Norwegian pyrites (Table I.) contain, before burning	1·649
,,	,, ,, ,, ,, after burning	0·465
	tons.	
100	Hard Norwegian pyrites make 140·875 $H^2 SO^4$, containing	1·481
140·875	Sulphuric acid make .... 104·9 HCl ,,	0·724
,,	,, ,, 204·12 $Na^2 SO^4$,,	0·059

In this Table the amounts are given in a manner more useful to manufacturers.

On analysis this deposit gave (as a mean of nine analyses) 43·4 per cent. arsenic trioxide. This flue had been in constant use for some years. (See Table IV.)

TABLE IV.

Flue from Salt-cake Furnace to Towers.

Deposit.

No. of analysis.	Arsenic trioxide.
	per cent.
1.	44·664
2.	42·936
3.	45·333
4.	39·982
5.	46·449
6.	44·398
7.	40·441
8.	38·977
9.	47·732
	390·912
Mean	43·4346

Coke in condensing-towers.—Although not expecting to find any arsenic in this, thinking that the water in the towers would completely decompose the chloride of arsenic which escaped from the reverberatory furnace, I made an analysis for it. For every analysis 10 lbs. of coke were employed, and digested first with pure distilled water and afterwards with perfectly pure hydrochloric acid. Arsenic was very easily detected in this solution. As an average of three analyses, I find 2·8 per cent. arsenic tri-

oxide, the variation being from 2·6 per cent. to 3·2 per cent. As^{2}O^3. It is difficult to imagine how this arsenic has been retained in the coke, as it would have been expected that the decomposition of the terchloride by the water in the towers would have been complete.. (See Table V.)

TABLE V.

Coke from Condensing-towers.

No. of analysis.	Arsenic trioxide.
	per cent.
1.	2·641
2.	3·182
3.	2·837
	8·660
Mean	2·886

Air in flue from condensing-towers to chimney.—Not only is the decomposition of the arsenic terchloride not completed in the towers, but a considerable amount of arsenic (in what condition I am not aware) escapes from the flue leading from the condensing-towers to the chimney of the works. The amount of air passing through this flue was 31,722 cubic feet per hour; and for each analysis 500 cubic feet were taken. The method employed for collecting the arsenic (trioxide?) contained in the air passing through this and the next mentioned flue was as follows:—The air was aspirated through three bottles containing respectively H^{2}O, HCl, and AgNO^3. The gas was allowed to bubble very slowly through the solutions; the bottles containing them were capable of holding 40 ounces, and were about half filled. It was found in most cases (it might almost be said in *all*) that the AgNO^3 solution was unnecessary, the H^{2}O and the HCl absorbing all the arsenic which was in the air drawn through.

As an average of twelve analyses, the following results were obtained:—

As^{2}O^3 per 1000 cubic feet.	As^{2}O^3 per hour.	As^{2}O^3 per day.
0·158 grain.	5·012 grains.	120·282 grains.

This amount may appear small; but when we consider that the 120 grains per day are from the chimney of one works only, the question becomes rather a serious one when we take into account the number of manufactories employed daily in throwing this amount into the air even within a short distance of Manchester. (See Table VI.)

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Table VI.

Flue leading from Condensing-towers to Chimney.

Amount of air taken for each analysis = 500 cubic feet.
Amount of air passing = 31,722 cubic feet per hour.

</div>

No. of analysis.	$As^2 O^3$ per 1000 cubic feet.	Amount per hour.	Amount per day.
	grain.	grains.	grains.
1.	0·068	2·157	51·768
2.	——	——	
3.	0·082	2·601	62·424
4.	0·072	2·284	54·816
5.	0·102	3·235	77·640
6.	0·064	2·030	48·720
7.	0·198	6·280	150·720
8.	0·248	7·867	188·808
9.	0·186	5·900	141·600
10.	0·232	7·359	176·616
11.	0·262	8·311	199·464
12.	0·382	12·117	290·808
	1·896	60·141	1443·384
Mean	0·158	5·012	120·282

Closely connected with this is the specimen of

Air taken 10 *feet from bottom of chimney.*—The same amount of air was taken for each analysis in this case as in the former, namely 500 cubic feet, the mean of nine analyses in this case being

<div align="center">

$As^2 O^3$
per 1000 cubic feet.
0·086 grain,

</div>

or nearly $\frac{1}{10}$ gr. per 1000 cubic feet. Surely such a state of things ought to be prohibited. The arsenious acid thrown into the atmosphere must in some places be very large. Of its danger there can be no doubt. But the great problem will be how to prevent its escape. It is owing to no carelessness on the part of the manufacturers; and it is one of those things that in our present state of knowledge it is scarcely profitable to remove. On whom must the blame rest?

Table VII.

Specimens of Air taken 10 feet from the bottom of Chimney.
Amount of air taken for each analysis = 500 cubic feet.

No. of analysis.	As^2O^3 per 1000 cubic feet.
	grain.
1.	0·046
2.	0·022
3.	0·086
4.	0·062
5.	0·112
6.	0·054
7.	0·122
8.	0·132
9.	0·144
	0·780
	·086

I hope to speak in another part of this paper on some of the methods employed for the removal of this "nuisance;" in the mean time I must proceed to speak of the remaining substances of alkali-manufacture. The last examined was the sodium sulphate; the next to undergo analysis was the

Sodium Carbonate.—This, up to the present time, has been found perfectly free from arsenical impurity; fifteen samples from twelve different works were submitted to analysis, and none was discovered.

Soda-waste.—In this the amount is comparatively trifling, giving as an average of six analyses 0·442 arsenic trioxide. (See Table II.)

Recovered Sulphur. Mond's process, before and after purification. —In specimens of sulphur recovered by Mond's process I have sometimes found great differences, the amounts varying sometimes from 0·442 to 0·901 per cent. arsenic trioxide; but its presence to this extent is only found in the unpurified samples. In those which have undergone purification none whatever is found, the average of four analyses of the unpurified sulphur being 0·7 per cent.

I think the foregoing analyses allow a simple and direct deduction to be drawn; and that is, if the arsenic is to be removed at all, every thing points to the sulphuric-acid stage as that in which the removal ought to take place. Sulphuric acid is the corner-stone of alkali-manufacture; cleanse it, and the whole is clean.

Section II. *Methods of removal.*

Two precautions have to be taken into account in the methods employed for the removal of arsenic from sulphuric acid.

1st. The substance or agent which is used in the purifying process must have no deleterious effect on any article in the manufacture of which the acid is required.

2ndly. We must prevent our works, as far as in us lies, from becoming a nuisance to our neighbours.

The item of expense is naturally a matter of course.

The following purifying agents have been carefully tried, with the following results.

Sulphuretted Hydrogen in a gaseous state.—This is, I believe, in use in many cases, but, as far as I can make out, with very variable results. For my own part I found it infringed the second requisite of a purifier, inasmuch as it became a "nuisance," and, which was much more serious, was too expensive. The plan employed in this case was a very simple one.

A large flat leaden pan was employed covered with a wooden top, and having access to a chimney by means of a long flue. About 20 feet from the pan this flue was led through a fire, which, decomposing the escaping sulphuretted hydrogen, not only prevented a great escape to the atmosphere, but allowed the deposited sulphur to be recovered.

Whether from defective draught, or from the unsuitability of the process to the required purpose, this plan did not answer; whilst the use of acid to liberate the gas, together with the difficulty in regulating the supply, made the expense of working too great.

Instead of using separate acid to liberate the sulphuretted hydrogen, I tried to use that which was to undergo purification, by the addition first of

Sulphide of Iron.—This plan is, of course, only admissible in certain cases. For acid employed in wire-working, galvanizing, or similar work, purification by the simple addition of ground sulphide of iron is quite safe, and completely answers the purpose required; whereas that containing arsenic cannot be used, its action having a deleterious effect upon the iron. Acid purified by this means, however, cannot be used in bleaching, dyeing, or printing; so that its field of usefulness is extremely limited; still, as a purifying agent, it was all that was required.

Sodium Sulphide.—As this was a substance capable of being used in most cases, more care was expended on it than on either of the former. The sulphide of sodium employed was made from black ash, and, although contaminated with a little lime, answered the purpose completely. The method of application was as follows :—

A known amount of sulphuric acid containing a determined percentage of arsenic trioxide was run into a large leaden pan, and a calculated amount of sodium sulphide added. At the bottom of this pan a layer of coke, which had previously been well digested with hydrochloric acid to free it from iron and other impurities, was placed, through which the acid, being run upon it, filtered, thus freeing it from the precipitated tersulphide, whilst it was run out from the pan by a tap at the bottom. The precipitate was removed from this filter every night; but the coke was allowed to remain for a long time, two or three months sometimes elapsing before removal. The escaping sulphuretted hydrogen was conveyed away by a process similar to that employed in the sulphuretted-hydrogen method.

The results obtained by this method were very satisfactory, and the expense was extremely moderate—one hundred gallons of sulphuric acid giving only a very minute trace of arsenic after being subjected to this process.

The deduction I naturally draw from the results of the above methods of purification is, that sulphuretted hydrogen can precipitate the last traces of arsenic in the acid it is required to purify. The only difficulty lies in the method to be employed, and the means of getting rid of the escaping sulphuretted hydrogen which has been allowed to be present in excess. It is necessary to state that the above method of decomposition of that gas did not fully answer the purpose required.

Sodium Chloride.—The next plan tried was purification by the addition of common salt to the acid. It was thus supposed that, according to the general rule, the arsenious acid would be converted into the terchloride of arsenic and escape as such. This, however, was found unsuitable, for many reasons.

In the first place, the decomposition of the salt is not perfect, a considerable amount remaining in the acid as sodium chloride. Next, its action upon the ordinary brown vitriol (as it runs from the chamber) was found not to be so perfect as upon that undergoing refining, so that it was necessary to add the salt to the glass retorts after they had been in action for some time. This, of course, entails great inconvenience. The necessity for opening the retort after the acid has been for some time in a state of ebullition is at once very disagreeable and very dangerous, whilst the sudden and powerful (partial) action upon the salt makes it an attempt not to be incautiously determined upon. If, on the other hand, the salt is introduced into the retort before the acid has commenced boiling, it collects in considerable amount at the bottom, and thus causes the mortality among the retorts to become a matter of some consideration, as well as making it a dangerous occupation for the man whose business it is to watch

them, as the bursting of such a retort over a fire carries with it great danger. This plan then was thrown aside. Although it could compete with any in cheapness, yet it carried too many inconveniences with it to become practicable. The following numbers may perhaps be interesting :—

Sulphuric Acid. As² O³ per cent. (before purification).		Sulphuric Acid. As² O³ per cent. (after purification).
1·131	contained	0·34
1·303	,,	0·48
0·991	,,	0·63

The above figures show that the decomposition was not perfect enough for the plan to be made use of on a manufacturing scale. I had found it to answer perfectly in my laboratory experiments, and so hoped that, when extended to the working scale, it would be equally successful.

Hydrochloric Acid.—A stream of hydrochloric acid gas was passed into the acid required to be purified, supposing the decomposition in this case would be similar to the above. This, however, was open to the same objections as the former, with the additional one that the expense entailed by the process is too great for any practical purpose.

In looking over the foregoing processes, one or two natural conclusions force themselves upon me. The process which, as far as I can yet see, we must rely upon for the purification of our acid is that which depends upon the use of sulphuretted hydrogen. Precipitation is much surer, and, indeed, more perfect, than decomposition. The latter is dependent upon too many conditions. The heat, the completeness of decomposition of the salt, the rapidity with which the liberated bubbles of hydrochloric acid gas pass through the acid, all exercise a great influence upon the success of the process. Whilst sufficiency of gas is the only requisite in the former case, the latter depends upon too many causes, too many conditions. Certainty in this case must be striven for before rapidity : once gain the former, and we may be sure of the latter following in its own good time. Besides, in the present condition of the law of the country in such matters, sulphuretted hydrogen is a much safer gas to throw into the atmosphere than hydrochloric acid !

I have thus given my own experience of the best methods of the purification of sulphuric acid, gathered during a pretty continued search after some sure and practicable method both in respect of efficiency and economy. I have chosen to do this rather than gather together the experiences of others—partly because these experiences are comparatively few and far between,

and partly because this matter has been looked upon by manufacturers as being of a merely secondary nature. But I was led to inquire into it in a more serious manner from the fact of having seen much acid returned to the manufacturers as being unfit for use owing to the presence (supposed!) of iron, while all the disturbance arose from the presence of this extremely difficult-to-be-got-rid-of substance, arsenic. In several instances also which have come to my notice in the cases of manufacturers of ammonium sulphate, many hundred pounds' worth of material has been lost through the use of acid containing this impurity, while the unfortunate makers were vainly searching for their old enemy, iron.

The importance of examination for the presence of arsenic can scarcely be too strongly urged upon the manufacturers of such materials. In conclusion let me say that, as far as my own experience goes, the use of sodium sulphide answers the purpose to a more perfect degree than any other process. Of course many improvements can be made upon the method of application given above; yet, for a rough but satisfactory method, I have found it sufficiently accurate.

XLV. *On an Experimental Determination of the Relation between the Energy and Apparent Intensity of Sounds of different Pitch. By* R. H. M. Bosanquet, *M.A., F.C.S., F.R.A.S., Fellow of St. John's College, Oxford.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen,

THE comparison of the intensities of sounds of different pitch has always been considered to present considerable difficulty; and the theoretical suggestions which have been made as to the measure of intensity have not been regarded as conclusive. The problem, however, is solved empirically every day; and the comparison of the intensity of sounds not differing widely in pitch presents no great difficulty.

The tones of every keyed instrument are so proportioned in power that in all parts of the range the effect may be as uniform as possible; and in the case of the organ, in which the power of each pipe is fixed once for all by the voicer, we have in each stop a graduated series of tones, the power of which has been adjusted to the production of a uniform effect, by the exercise of a skill more easy to admire than to imitate.

Strictly speaking, the effort of an organ-voicer is to make the more acute tones so far prominent above the lower ones, that the higher may be clearly heard through the lower. That the

effect sought is really a predominance of the upper notes, appears as well from the statement of the voicers as from some of the means employed to produce the desired effect. Thus one organ-builder uses two pipes instead of one throughout his trebles; others employ a heavier wind in the treble; so that we must allow that in organ-stops the intensity of the treble is required slightly to exceed that of the bass. But, allowing for a variation of this nature (namely, a slight excess in the intensity of the treble over that of the bass), it seems reasonable to assume that the apparent intensity of the different notes of a well-voiced organ-stop is the same.

The work consumed in an organ-pipe in a given time is measured by the amount of wind it draws from the bellows in that time.

There is a statement in Töpfer's work on the organ (1842, reprinted 1862, Körner, Erfurt and Leipzig), that when a stop is scaled according to his measures, and properly voiced, the wind-consumption of the pipes is proportional to their lengths, or to the wave-lengths: this law is stated without reasons or measures; so that some verification was essential; but before passing to the verification I will notice the immediate consequences of the law, if true.

The quantities of wind consumed by the pipes in a given time are measures of the work done by the weights on the bellows. We have to assume that in similar pipes the work converted into sound is a constant proportion of the whole work supplied; for undoubtedly loss takes place in the pipes. Subject to this, we derive from Töpfer's law the statement that the work converted into sound by the pipes of an organ-stop in a given time is proportional to the wave-length of the tone, or the periodic time.

Admitting, then, that the notes of an organ-stop are fair representatives of a series of tones of different pitch and equal apparent intensity, we have more generally:—Tones of different pitch have equal apparent intensity when the work consumed by them in a given time is proportional to the wave-length or periodic time; or if W is the work consumed in a unit of time, the intensity is measured by the fraction $\dfrac{W}{\tau}$, where τ is the periodic time—assuming that the intensity in notes of the same pitch is proportional to W, which will not be denied.

For the purpose of obtaining some verification of Töpfer's law, I employed the new open diapason on the great organ of the instrument in St. John's Chapel, Oxford; it draws wind enough to observe conveniently as far up as about the top of the treble staff, and is tolerably uniform in tone. Some difficulty

was found in following out Töpfer's method of observation with
accuracy, until the minutiæ which follow were attended to. The
object is to observe the time of descent of the bellows under dif-
ferent circumstances. A pendulum of bullet and string was
employed to observe the time, by counting the oscillations; the
eye was thus occupied. The points of full and empty were de-
termined by the feel of the bellows-handle, as follows:—The
excess-valve is placed between the bellows and one of the feeders;
so that, when the bellows is overfull, the pressure of the wind
comes on the feeder, and destroys the equilibrium of the handle,
causing a downward pressure, which is sustained by the hand.
At the moment of closing of the excess-valve this pressure dis-
appears, and thus the moment of departure of the observation is
distinctly announced. A similar sudden change of pressure in-
dicates the emptying of the bellows and close of the observation.
Until this method was adopted it was impossible to obtain the
accuracy required; but with it the accuracy was great, so that,
if the same element was observed repeatedly, the number came
almost always the same.

As a preliminary experiment, I observed some time ago the
wind-consumption of the five C pipes; the results were as
follows:—

	$a.$	$b.$	$\frac{1}{b}-\frac{1}{a}=\mathrm{W}.$	$\frac{\mathrm{W}}{\tau}.$	$\frac{\mathrm{W}}{\tau}-49\cdot3.$
c	95	59	641	40·0	
c_0	92	69	406	50·7	$+1\cdot4$
c^{l}	93	79	191	47·7	$-1\cdot6$
c^{ll}	100	91	99	49·5	$+0\cdot2$
c^{lll}	96	91	57	57	

The first column contains the name of the key depressed. a is
the number of oscillations of the pendulum counted during de-
scent of bellows with stop closed. b is the number of oscillations
during descent of bellows, pipe sounding. $\frac{1}{a}$ and $\frac{1}{b}$ are conse-
quently the fractions of the content of the bellows consumed in one
oscillation in the two cases, and $\frac{1}{b}-\frac{1}{a}$ is the fraction of the con-
tent of the bellows consumed by the pipe alone in one oscillation of
the pendulum; and this I have taken as the measure of the work
W converted into sound during one oscillation of the pendulum.
$\frac{\mathrm{W}}{\tau}$, where τ is referred to the periodic time of c^{lll} as unity, is
the next column; it should be constant according to Töpfer's
law, and is the measure of intensity according to what precedes.
The number for c is seen to depart widely from the result of

the three middle observations, which agree closely. This is easily explained, and the rejection of the observation for our purposes justified as follows:—The largest bass pipes of the manual in an organ have almost unavoidably a deficient supply of wind. In consequence of the heaviness of the touch caused by large valves, the valves and, therefore, the wind admitted by them are limited in size and quantity respectively; and as this limited amount of wind has to be distributed amongst a number of large pipes, each has to put up with less than a full supply; and it is well known that the power of the bass invariably suffers more or less in consequence of this difficulty.

The number for c''' is rejected because the quantity to be observed is in that case too small, with reference to the method of observation, for accurate results.

So far as the above slight observations went, they appeared, then, to corroborate Töpfer's law. But with the view of ascertaining more exactly how far the uniformity sought for prevails, I subsequently observed the wind-consumption of each pipe from tenor C (c_0) to treble C (c'') inclusive. Several months had elapsed and the organ been tuned, which must account for the difference in the relative behaviour of the C pipes on the two occasions.

The results are as follows:—

	a.	b.	$\frac{1}{b}-\frac{1}{a}=\mathrm{W}$.	$\frac{\mathrm{W}}{\tau_{c''=1}}$.				
c_0	143	111	201	50·2		+6·3		+ 7·3
$c_0\sharp$	153	123	159	42·1		−1·8		− 0·8
d_0	159	125	171	48·0	W | ♭ — 43·9	+4·1		+ 5·1
$e_0\flat$	159	128	152	45·2		+1·3		+ 2·3
e_0	149	127	115	36·4		−7·5	W | ♭ — 42·9	− 6·5
f_0	158	132	124	41·4		−2·5		− 1·5
$f_0\sharp$	159	137	101	35·7		−6·2		− 6·2
g_0	152	130	111	41·6		−0·3		− 1·3
$a_0\flat$	148	129	99·5	39·3	W | ♭ — 41·9	−2·6		− 3·6
a_0	160	134	121	50·9		+9·0		+ 8·0
$b_0\flat$	166	126	191		W | ♭			
b_0	152	132	99·7	46·7		+4·8		+ 3·8
c'	154	138	75·3	37·5		−4·4		− 5·4
$c'\sharp$	163	143	85·8	45·4		−2·6		− 1·8
d''	168	145	94·5	53·0		+5·0		+ 6·8
$e'\flat$	161	141	88·1	52·4	W | ♭ — 48·0	+4·4		+ 5·2
e'	162	145	72·4	45·6		−2·4		− 1·6
f'	166	149	68·7	45·8		−2·2		− 1·4
$f'\sharp$	165	149	65·0	45·7		−2·3	W | ♭ — 47·2	− 1·5
g'	162	150	49·4	37·0		−9·4		−10·2
$a'\flat$	170	154	61·2	48·6		+2·2		+ 1·4
a'	168	152	62·7	52·7	W | ♭ — 46·4	+6·3		+ 5·5
$b'\flat$	164	152	48·1	42·8		−3·6		− 4·4
b'	162	149	53·8	50·8		+4·4		+ 3·6
c''	171	158	46·9	46·9		+0·5		− 0·3

The error of the method of observation may be entirely disregarded; the deviations from uniformity are to be taken as resulting from irregularities in the proceedings of the voicer.

Collecting the results, we have :—

$\dfrac{W}{\tau}$.		$\dfrac{W}{\tau}$.	
c_0-f_0	$43 \cdot 9 \pm 1 \cdot 37$	c_0-c'	$42 \cdot 9 \pm 1 \cdot 0005$
$f_0^{\#}-c'$	$41 \cdot 9 \pm 1 \cdot 60$		
$c^{\#}-f'^{\#}$	$48 \cdot 0 \pm 1 \cdot 01$	$c'^{\#}-c''$	$47 \cdot 2 \pm 0 \cdot 932$
$g'-c''$	$46 \cdot 4 \pm 1 \cdot 57$		

The probable errors of these arithmetic means are appended. From the near equality of these in the results of the two separate octaves, we infer that the estimation by the ear of equal intensities of tone has about the same accuracy whatever the pitch may be.

This discussion establishes almost beyond a doubt the existence of the law of Töpfer as a basis; but the results are affected by irregularities of a more continuous nature than are to be expected in ordinary cases. The upper half of the tenor octave, for instance, furnishes the mean 41·9, which deviates widely from all the rest. If, however, we call to mind the nature of the estimation of intensities (which takes place for the most part by comparison of adjoining notes), we see that an error once committed is liable to perpetuate itself; and therefore precisely this description of continuous irregularity is to be expected. If we admit the existence of an error of this kind in the upper half of the tenor octave, and if we admit that, according to the practice of voicers, some increase in strength is looked for on entering the octave above middle *c*, we completely account for the discrepancies in the results.

The $b_0\flat$ (which is omitted in the above discussion) has a wind-consumption quite disproportionate to all the rest; the tone of the pipe indicates a difference in the voicing: a mouth cut up a good deal higher than usual would account for both effects; a pipe with a high mouth takes much more wind to reach the upper lip properly.

On the whole, the law of Töpfer is not supported by this discussion as an accurate expression of the properties of the organ-stop examined; but the very deviations from this law which occur in the above figures support the proposition that $\dfrac{W}{\tau}$ is a measure of the apparent intensity of the tone.

If I be the apparent intensity of a tone of periodic time τ, we have seen that it is measured by $\dfrac{W}{\tau}$; if, then, w be the constant proportion of W (say $\dfrac{W}{p}$) which reaches a thin plate

of air at the orifice of the ear in a unit of time, we may put
$I = h \dfrac{w}{\tau}$.

Here a certain proportion of the energy expended traverses, at right angles we suppose, a small plate of air whose mass we will assume constant and equal to unity. Then the air is subjected to a forced vibration, which may be represented by $x = a \sin nt$ in the simplest case; and the energy of this vibration is measured by $\dfrac{a^2 n^2}{2}$, where $n\tau = 2\pi$; that is to say, $\dfrac{2\pi^2 a^2}{\tau^2}$ measures the energy communicated to the plate of air in a quarter of a vibration.

It is clear that if we take the expression for the velocity

$$\frac{dx^2}{dt^2} = n^2(a^2 - x^2),$$

where $n\tau = 2\pi$, we cannot avoid having the square of the periodic time in the denominator of the *vis viva*. In a recent paper in the Philosophical Magazine, Mr. Moon has omitted this element. See the determination of the energy of a vibrating string, Donkin's 'Acoustics,' p. 127, which is to some extent analogous to the present case, except in the difference of the vibrating body.

If the plate were moving freely, the energy gained as kinetic in the first quarter oscillation would be transformed into the form of potential in the second; but here we have no potential energy in the plate of air, the movement at every instant depending on what is transmitted from the source of energy. Thus all the elements of work in both directions proceed from the source, and must be reckoned in estimating its action on the air: this observation occurs in Mr. Moon's paper.

The work passed through the plate of air in a whole oscillation is then
$$\frac{8\pi^2 a^2}{\tau^2}.$$

Hence the following proposition :—In notes whose oscillations are similar, *i. e.* have the amplitudes proportional to the wavelengths or periodic times, the work transmitted by one oscillation of each note is the same.

Since there are $\dfrac{1}{\tau}$ oscillations in a unit of time,
$$w = \frac{8\pi^2 a^2}{\tau^3}.$$

Substituting this for w in the expression for the intensity, we have
$$I = h \cdot \frac{8\pi^2 a^2}{\tau^4},$$

whence proceed the following propositions :—

In notes of different pitch the apparent intensity varies as the

square of the amplitude, and inversely as the fourth power of the wave-length or periodic time.

In series of notes of equal apparent intensity, but of different pitch, the amplitudes vary as the squares of the wave-lengths or periodic times.

In some works which allude to this subject, a confusion exists between the energy of a vibrating instrument, as a string, and the intensity of the tone produced thereby. It is necessary to remember that the energy of the instrument has nothing to do with the tone heard, as it is only that portion of the energy which is lost to the instrument by being communicated to the air that causes the spread of the tone. Thus the energy of a string set in vibration is gradually exhausted by communication to the air, and the energy of the string does not represent the apparent intensity of the tone at any instant, but the total energy expended during the duration of the tone until it dies away. Again, in an organ-pipe there is reason to believe that the energy actually stored up in the vibrations of an organ-pipe in steady motion is considerable; in fact it goes on storing until the communication of energy to the outer air equals the accession constantly received from the source of power. Thus the energy of vibration within the pipe may be much greater than that which, so to speak, flows through it. We have neglected in the foregoing the energy of the pipe itself, and confined our attention to that supplied to it, which in steady motion is necessarily equal to that parted with to the surrounding air. The result is then different from that propounded heretofore; for the energy of a pipe or string necessarily contains the integral of the motion throughout the vibrating mass, a quantity with which the energy which passes out has nothing to do. As another illustration, the energy of a tuning-fork sharply struck is considerable; that of the vibrations excited in the air around is in comparison evanescent. When the impulse in this case is constantly renewed in Helmholtz's machine by the electromagnetic apparatus there applied, and the tone taken over by a resonator into the surrounding air, we have another example of what has been described in the case of the organ-pipe: the store of energy of the fork is quite independent of the flow of energy from the electromagnet into the resonator; and in estimating the apparent intensity of the tone, the latter alone is to be regarded.

In answer to the note on the second page of Mr. Moon's paper, as to the law of the intensity in optics, it is enough to observe that the law of Malus, connecting the intensities of the ordinary and extraordinary rays in doubly refracting crystals, affords an experimental confirmation of the law of the squares of the amplitudes, for waves of the same period.

XLVI. *Notices respecting New Books.*

The Orbs around us: a Series of familiar Essays on the Moon and Planets, Meteors and Comets, the Sun, and coloured Pairs of Suns. By Richard A. Proctor, *B.A.* (*Camb.*) London: Longmans, Green, and Co. 1872.

THERE are two classes of scientific authors—one consisting of men who chronicle the results of their researches obtained by the aid of observation or experiment, the other of men who, gathering up these results, present them to the public in a pleasing and attractive form. In each of these classes we find men who are justly entitled to be regarded as "the leaders of scientific thought," and from the general character of the now numerous works that have issued from the pen of Mr. Proctor, the position of "leader of *popular* astronomic thought" may be unhesitatingly accorded to him.

The work before us contains a series of essays originally published in 'St. Paul's,' 'Fraser's,' and the 'Cornhill' Magazines, on spectrum-analysis, the habitability of the planets Venus, Mars, and Jupiter, meteor and meteor-systems, comets and their tails, the sun's corona, and the colours of double stars. Most of the subjects are treated in a lucid and familiar style, particularly that of spectrum-analysis, under the title of "The Gamut of Light," and also that of "Meteor Systems." Musical readers who possess an acquaintance with the science of sound will have no difficulty in following our author in his exposition of the phenomena of the spectrum, "The Gamut of Light;" and his exposition of the connexion between meteor-systems and comets will at once commend itself to the thoughtful reader who can appreciate the difficulty that must have been experienced by the eminent mathematicians who worked out the orbits, and who showed the close connexion between the two great meteor-systems now known to belong to the solar system, and the two comets with which they are intimately associated. Occasionally our author departs from his more popular style, indulging in one somewhat more abstruse, particularly in the enunciation of theories which he has originated; and we find in several instances a spice of the fanciful, bordering on the sensational, as in such titles as "the planet of love," "the planet of war," and a "miniature sun,"—also in the very frequent use of the word "startling," as if the sober realities of science were so far removed from the general track of ordinary thought that the unravelling of the symmetrically woven web of natural knowledge should be accompanied by sensations different from that of a deep feeling of admiration at the power given to man to read, mark, learn, inwardly digest, and *understand* the great book of Nature spread open before him.

Viewing the book as the production of a recognized leader of popular astronomic thought, we have marked a few passages in which we apprehend the author has fallen short of his high vocation. Speaking of the attempt to secure clearness of illustration with strict scientific exactness, he says, "Scientific exactness can come afterwards," &c. In one or two instances the author has followed his own rule (as we conceive) detrimentally. He tells us that Dr.

Huggins has proved that Sirius is receding from us at the rate of several hundred miles per second. Now Dr. Huggins has done nothing of the sort; his earliest determination of the motion of Sirius before Mr. Proctor wrote his essay was twenty-five miles per second; he has since found reason to reduce the velocity to eighteen, or at most twenty-two miles per second. Another instance is, that the equatorial belts of Jupiter and Saturn are in no sense comparable with the zone of calms or *doldrums*—in their being persistent, whereas our zone of calms travels far north of the equator in summer, and far to the south in winter. We were not aware, until thus enlightened by Mr. Proctor, that a zone about 6° broad, its *mean* position being north of the equator with its northern edge somewhere near the parallel of 8°, was removed *far* from this position; perhaps his readers would have been really more enlightened had the two limits of 15° N. and 5° S. been given; and we think that in both the cases alluded to " clearness of illustration " would not have been sacrificed by giving these quantities with scientific exactness.

On the subject of " other habitable worlds," much as has been written, we cannot possibly obtain any definite knowledge, beyond that of ascertaining *by observation* the conditions of *habitability* of " the orbs around us." One, and only one, has yielded us any definite information on this head. It is now seven years since Professor Phillips constructed a map of Mars, and showed, from the oscillations of the snow-zones of the planet, that its climatic relations are similar to those of the earth—in other words, that it is a suitable residence for beings of a similar constitution to the human race. One or two quotations from Phillips occur in Mr. Proctor's description of Mars; but the map is passed over unnoticed, although we have a map constructed by our author from drawings by the late Mr. Dawes. A comparison of the two is very suggestive. In the essay entitled "The Rosse Telescope set to New Work," we find large reflecting telescopes described as inadequate to present objects in a perfectly distinct manner, their value consisting in their light-grasping powers. The new work of the Parsonstown reflector is that of testing its heat-grasping powers; and in this it has been successful in furnishing us with a measurable amount of heat derived (or rather reflected) from the moon; and this result our author characterizes as " Lord Rosse's discovery." It appears that Mr. Proctor must have forgotten, or he would certainly have referred to, Professor Piazzi Smyth's work in this direction, recorded on pages 212, 213 of the account of his celebrated astronomer's experiments. It is, however, quite possible that this particular passage escaped his attention; but what are we to think of the following? In his essay on shooting-stars, alluding to the theory of meteors being propelled from the moon, he speaks of the inadequacy of the force of propulsion to give them their observed velocities, " even," he says, " if it were proved (which is far from being the case) that any active volcanoes now exist in the moon." This was published in the ' Cornhill Magazine ' for November 1867. We have now before us the Number of ' Temple Bar,' published only three months earlier, viz. in August 1867; and in it we find an article headed, " A Lunar

Volcano in Eruption, by Richard A. Proctor, B.A., F.R.A.S., author of ' Saturn and its System, &c.," in which the author is at some pains to establish the then *supposed* change on the moon's surface, in the celebrated case of the Crater Linné. We can hardly be mistaken in the authorship of this paper ; for the probabilities are immense against two men bearing the same name and writing on the same subject. Now what does the Mr. Proctor of ' Temple Bar ' say of this *supposed* change ? We would quote the passage at length did space permit. Suffice it to observe that he speaks of an *actual* change having taken place, which he explains by a mass of matter having been poured into the crater from below, overflowing its barrier, and covering the steep sides of the former ring, and commences his concluding paragraph with these words: " For the first time, then, after so many years of patient labour, we have undoubted evidence of change upon the moon's surface." His closing words are, " The most sceptical must accept the combined evidence on this interesting point as absolutely decisive." This looks very like a proven case on the part of the author.

There are a few other records of some importance which appear to have escaped Mr. Proctor's attention. Our space will not allow further notice than merely to refer to the unique view of Venus in the last century, when the surface of the planet appeared to be indented with spots similar to those on the moon.

In Mr. Proctor's remarks on Jupiter as " a miniature sun," our author calls attention to the absence of any thing in the meteorology of the earth at all comparable with the mutual disturbances of the Sun and Jupiter by each other, synchronous with the " Sun-spot period." This, it appears, was somewhat premature. Professor Meldrum, from a study of the frequency of the cyclones of the Indian Ocean, has succeeded in establishing a periodicity of these phenomena synchronous with the " Sun-spot period." We are now, therefore, not warranted in concluding that the meteorology of Jupiter *alone* suffers disturbances originating in the sun ; for Professor Meldrum's discovery establishes that our earth sympathizes with both.

Setting aside the instances to which we have alluded as indicating a want of sufficient care by the author in the composition of his pleasing and lucid articles, we consider the book well calculated for its office—that of spreading a knowledge of astronomy among ordinary readers ; and in taking leave of it, we may remark that we have derived much pleasure from its perusal, and hope the author will take an early opportunity of placing before his readers, in his usual familiar style, the omissions we have mentioned.

The Geometry of Conics.—Part I. *By* C. Taylor, *M.A., Fellow of St. John's College, Cambridge.* Cambridge : Deighton, Bell, and Co. London : Bell and Daldy. 1872. (8vo, pp. 88.)

We do not find that the author any where states what will be contained in the other part or parts of this work, except incidentally that orthogonal Projections and the sections of the Right Cone will be " discussed in sequel." This first part contains a complete ac-

count of the chief properties of the Parabola, Ellipse, and Hyperbola considered as distinct curves. Under the head of the Ellipse, however, the author has distinguished those properties which belong to the Conics in common. He states that the work is the " result of an attempt to reduce the chaos of Geometrical Conics to order," and that he has " endeavoured to reconstruct it on a uniform plan, taking as a standard, whereby to regulate the sequence of the proofs, the principle that *Chord properties* should take precedence of *Tangent properties*." Accordingly each curve is discussed in two chapters, the former treating of its chord properties, the latter of its tangent properties. *E. g.* in the case of the parabola two simple proofs are given of the proposition that the locus of the middle points of any system of parallel chords is a straight line parallel to the axis ; and that the bisecting line meets the directrix on the straight line through the focus perpendicular to the chords. From this the relation $QV^2 = 4SP \cdot PV$ is easily proved. Before passing to the tangent properties the author mentions, with brevity and clearness, several ways in which a chord may be made to assume a position of tangency. One marked peculiarity of the book is the detailed treatment of the rectangular hyperbola—a curve which stands to the hyperbola in the same relation as the circle stands to the ellipse ; its numerous properties are deduced with great simplicity. The book, as a whole, is not easy to describe : it consists, as all such books must consist, of proofs of a number of well-known properties of the Conic Sections ; its merit lies in the arrangement of the propositions and the way in which they are proved ; but this merit can hardly be duly appreciated unless the book be compared page by page with one of the older treatises on the same subject. It is designed for the use of rather a high class of students, and will meet their wants admirably, both in regard to the text and the numerous examples and exercises with which it is furnished.

The Laws of the Winds prevailing in Western Europe.—Part I. *By* W. Clement Ley. London : E. Stanford. 1872.

In this work, which is an admirable companion to the Weather Maps of the Meteorological Office, Mr. Ley has treated a subject of no little difficulty and complexity with great clearness and perspicuity. Those of our readers who daily consult the Weather Maps are doubtless familiar with the " baric depressions " which are every now and then specified as travelling eastward. These meteorological phenomena the author has specially examined, the data being the " daily weather reports " from numerous stations in Western Europe, and finds that they are associated invariably with *extensive* areas of precipitation of aqueous vapour, in the central portions of which ascending columns of air are induced, and to which an indraught is established. This indraught by the rotation of the earth is so modified as to produce circulatory currents or winds of a cyclonic character, moving in a direction contrary to that of the hands of a watch. This circulatory movement brings successively over a tract of country two oppositely conditioned winds, the south-westerly in advance of the baric minimum, or *lowest* barometer, laden with aqueous vapour from

the Atlantic, and the north-easterly and north-westerly, very much drier, which succeed the passage of the minimum. By the precipitation of the vapour of the south-westerly winds a new area of precipitation is formed to the eastward of the initial area, accompanied by its baric minimum; and thus an apparent travelling of the minimum and its accompanying baric depression is induced, the direction varying with the season; it is also greatly modified by the general distribution of land and water. These are the principal features of the work before us, which we strongly recommend to meteorologists as indicating a course of study which in our opinion must contribute to the advancement of Meteorology. While the Meteorological Committee of the Royal Society specifies in the maps issued under its auspices the "baric depressions" which pass over Ireland, Great Britain, and the north-west of Europe, and so far brings an important meteorological phenomenon into view, we think that an extension of Mr. Ley's work by the Office—in classifying for each month in the year the paths of these depressions, and showing to a greater extent than he has done their relations to season and also to the configuration of land and water—would greatly contribute to the improvement of the data on which storm-signals are ordered to be hoisted, and would result in a greater percentage of the *justified* orders than obtain at present.

XLVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 316.]

June 20, 1872.—Sir James Paget, Bart., D.C.L., Vice-President, in the Chair.

THE following communication was read:—

"Preliminary Note on the Reproduction of Diffraction-gratings by means of Photography." By the Hon. J. W. Strutt, M.A.

During the last autumn and winter I was much engaged with experiments on the reproduction of gratings by means of photography, and met with a considerable degree of success. A severe illness has prevented my pursuing the subject for some months, and my results are in consequence still far from complete; but as I may not be able immediately to resume my experiments, I think it desirable to lay this preliminary note before the Royal Society, reserving the details and some theoretical work connected with the subject for another opportunity.

It is some years since the idea first occurred to me of taking advantage of the minute delineating power of photography to reproduce with facility the work of so much time and trouble. I thought of constructing a grating on a comparatively large scale, and afterwards reducing by the lens and camera to the required fineness. I am now rather inclined to think that nothing would be gained by this course, that the construction of a grating of a given number of lines and with a given accuracy would not be greatly facilitated by enlarging the scale, and that it is doubtful whether photographic or other lenses are capable of the work that would be required of them.

However this may be, the method that I adopted is better in every respect, except perhaps one. Having provided myself with a grating by Nobert, with 3000 lines ruled over a square inch, I printed from it on sensitive dry plates in the same way as transparencies for the lantern are usually printed from negatives.

In order to give myself the best chance of success, I took as a source of light the image of the sun formed by a lens placed in the shutter of a dark room. I hoped in this way that, even if there should be a small interval between the lines of the grating and the sensitive surface, still a *shadow* of the lines would be thrown across it. Results of great promise were at once obtained, and after a little practice I found it possible to produce copies comparing not unfavourably with the original. A source of uncertainty lay in the imperfect flatness of the glass on which the sensitive film was prepared, though care was taken to choose the flattest pieces of patent plate. The remedy is, of course, to use worked glass, which is required in any case if the magnifying-power of a telescope is to be made available.

Almost any of the dry processes known to photographers may be used. I have tried plain albumen, albumen on plain collodion, and Taupenot plates. The requirements of the case differ materially from those of ordinary photography, sensitiveness being no object, and hardness rather than softness desirable in the results. After partial development, I have found a treatment with iodine, in order to clear the transparent parts, very useful. In proceeding with the intensifying, the deposit falls wholly on the parts that are to be opaque. It is more essential that the transparent parts should be quite clear than the dark parts should be very opaque.

The performance of these gratings is very satisfactory. In examining the solar spectrum, I have not been able to detect any decided inferiority in the defining-power of the copies. With them, as with the original, the nickel line between the D's is easily seen in the third spectrum. I work in a dark room, setting up the grating at a distance from the slit fastened in the shutter, and using no collimator. The telescope is made up of a single lens of about thirty inches focus for object-glass, and an ordinary eyepiece held independently. I believe this arrangement to be more efficient than a common spectroscope, with collimator and telescope all on one stand; at any rate, the magnifying-power is considerably greater, and it seems to be well borne.

I have also experimented on the reproduction of gratings by a very different kind of photography. It will be remembered that a mixture of gelatine with bichromate of potash is sensitive to the action of light, becoming insoluble, even in hot water, after exposure. In ordinary carbon printing the colouring-matter is mixed with the gelatine and the print developed with warm water, having been first transferred so as to expose to the action of the water what was during the operation of the light the hind surface. In my experiments the colouring-matter was omitted, and the bichromated gelatine poured on the glass like collodion and then allowed to dry in the dark. A few minutes' exposure to the direct rays of the sun then sufficed to

produce such a modification under the lines of the gratings that, on treatment with warm water, a copy of the original was produced capable of giving brilliant spectra. In these gelatine-gratings all parts are alike transparent, so that the cause of the peculiar effect must lie in an alternate elevation and depression of the surface. That this is the case may be proved by pressing soft sealing-wax on the grating, when an impression appears on the wax, giving it an effect like that of mother of pearl. It is known that the effect of water on a gelatine print is to make the protected parts project in consequence of their greater absorption, but it might have been expected that on drying the whole would have come flat again. It is difficult to say exactly what does happen; and I am not even sure whether the part protected by the scratch on the original is raised or sunk. Gelatine can scarcely be actually dissolved away, because the uppermost layer must have become insoluble under the influence of the light. I do not at present see my way to working by transfer, as in ordinary carbon printing.

I have not yet been able to reduce the production of these gelatine-gratings to a certainty, but can hardly doubt the possibility of doing so. One or two of considerable perfection have been made, capable of showing the nickel line between the D's, and giving spectra of greater brightness than the common photographs. Not only so, but the gelatine copy surpasses even the original in respect of brightness. The reason is that, on account of the broadening of the shadow of the scratch, a more favourable ratio is established between the breadths of the alternate parts.

Theory shows that with gratings composed of alternate transparent and opaque parts the utmost fraction of the original light that can be concentrated in one spectrum is only about $\frac{1}{10}$, and that this happens in the first spectrum when the dark and bright parts are equal. But if instead of an opaque bar stopping the light, a transparent bar capable of retarding the light by half an undulation can be substituted, there would be a fourfold increase in the light of the first spectrum. I accordingly anticipate that the gelatine-gratings are likely to prove ultimately the best, if the conditions of their production can be sufficiently mastered.

With regard to the application of the photographs, I need not say much at present; it is evident that the use of gratings would become more general if the cost were reduced in the proportion, say, of 20 to 1, more particularly if there were no accompanying inferiority of performance.

The specimens sent with this paper are both capable of showing the nickel line and give fairly bright spectra, but they must not be supposed to be the limit of what is possible. From their appearance under the microscope I see no reason to doubt that lines 6000 to the inch can be copied by the same method, a point which I hope shortly to put to the test of experiment.

XLVIII. *Intelligence and Miscellaneous Articles.*

ON THE ANOMALOUS DISPERSION EXHIBITED BY CERTAIN SUBSTANCES. BY M. J. L. SORET.

M. CHRISTIASEN and M. Kundt have recently published some very remarkable experiments upon the anomalous dispersion that certain substances, such as the aniline colours, permanganate of potash, and others, exhibit when examined in concentrated solutions. A prism formed of these fluids between two plates of glass shows a spectrum wherein the sequence of the colours differs from that of ordinary substances, blue and violet being less refracted than red. When performed in this manner the experiment presents a certain difficulty, inasmuch as the fluids are of a very deep colour, and the light can therefore pass through but a very limited thickness thereof, so that the pencil can only be allowed to traverse the fluids quite close to the edge of the prism.

The importance of the phenomenon in question will not have escaped the notice of any physicist; and I therefore conceive it may prove of some interest to specify a consequence thereof that may be rendered evident by an experiment which can be repeated without difficulty. It consists in the inversion of the spectrum. This is effected by putting the solution under examination into a hollow prism of about 30°, and placing this prism in a cell with sides of parallel glass, which is filled with the fluid used for forming the solution of the substance with anomalous dispersion experimented upon. One can then observe the inverted spectrum in a less concentrated, and consequently more transparent solution, than when the prism remains in air. I will cite a few instances.

Fuchsine, or *Magenta*.—Take a spectroscope, and having removed the ordinary prism, replace it by a hollow prism filled with a concentrated solution of fuchsine. If one employs an intense pencil of light falling very near the edge of the prism, one succeeds in getting a sight of the inverted spectrum without having recourse to the cell spoken of above—that is to say, when the prism is left in air. With a solution of fuchsine that is only moderately concentrated the spectrum is normal, or, in other words, the red is less refracted than the violet. With a solution of intermediate strength, the spectrum becomes reduced almost to a single bright red-coloured band; in this case the inverted dispersion due to the fuchsine is almost exactly balanced by the opposite dispersion brought about by the alcohol employed as the solvent; we have deflection without dispersion.

If we now remove the prism filled with this latter solution into the cell containing alcohol, the general deflection of the rays is nearly entirely annulled, while the anomalous dispersion of the fuchsine remains; the deflection of the red exceeds that of the violet. It is no longer required to employ so intense a light, or to let it fall close to the edge of the prism.

On measuring the angle of deflection for the solution in question, I found, when the prism was in air, that it amounted, for the red band, to about 11° 30′, but when put into alcohol there was scarcely

any deflection of the violet, while that of the red was 15′, and that of the orange-yellow 23′. With a far less concentrated solution of fuchsine the deflection of the violet was also nearly imperceptible, while that of the red was 6′ and of the orange-yellow 16′.

Hence, for the violet rays, solutions of fuchsine have almost the same refractive index as alcohol, but for the red rays a higher one.

Aniline violet.—With an aqueous solution of this substance, and with the prism situated in air, I obtained a normal spectrum, wherein all the colours were visible ; the deflection of the red was 10° 24′, and that of the violet 10° 43′. On placing the prism in the cell filled with water, the spectrum became reduced to two bands (one blue, and one of a carmine red), which overlapped when the slit of the spectroscope was not extremely narrow. When sunlight was made use of, there was, in addition, a trace of green towards the end of the spectrum at the side of the blue band. The deflection of the blue amounted to 1′, that of the red to 4′.

Permanganate of Potash.—When filled with a solution of permanganate of potash, the prism, in air, gave a normal spectrum, wherein the deflection of the red was 10° 33′, and that of the violet 10° 53′. When placed in water, the prism gave, for violet, a deflection of 6′, for red of 9′, and for yellow of 12′.

From these numbers we perceive how the addition of substances having anomalous dispersion lessens the dispersive power of the solvent without materially affecting its mean refractive index. If one goes on increasing the strength of the solution, the dispersive power becomes at first zero, and then negative. The experiment with the prism surrounded with the solvent is of higher interest with the substances last named than in the case of fuchsine, inasmuch as aniline violet and permanganate of potash must be used in extremely concentrated solutions if they are to exhibit an inverted spectrum with the prism in air. In this case the observation is far more difficult to make than with fuchsine.

The papers of Messrs. Christiasen and Kundt, referred to above, are to be found in Poggendorff's *Annalen*, vol. cxliii. p. 250, vol. cxlii. p. 163, and vol. cxliii. pp. 149 & 259. Vol. cxliv. p. 128, vol. cxlv. pp. 67 & 164, vol. cxlvi. p. 154 contain further papers on the same subject.—Poggendorff's *Annalen*, vol. cxliii. p. 325.

ON THE MEASUREMENT OF THE INTENSITY OF CURRENTS BY MEANS OF THE ELECTROMETER. BY M. E. BRANLY.

When a wire is traversed by a voltaic current, its different sections present different charges of static electricity. A and B being two points in the conductor, a the electric potential in A, b the potential in B, the difference $a - b$ is proportional to the intensity of the current. This is expressed by Ohm's formula

$$i = C \frac{a - b}{r}.$$

C is a constant ; r represents the resistance of the part AB. If the wire is cylindric and homogeneous, the electrical density at each

point (quantity of electricity on the unit of surface) is proportional to the potential.

The proportionality between the difference of potential and the intensity is easy to verify by means of the mirror-electrometer which I described in a communication on the 19th of February last. One of the pairs of sectors of the electrometer takes the same potential as A, and a quantity of electricity proportional to that potential; the other pair takes the same potential as B.

Between the poles of a pile, liquid or metallic resistances were interposed; Pouillet's tangent-compass gave the intensities for the strong currents, Weber's for the feeble ones. The electrometer measured the differences of potential or tension * corresponding to the extremities of various coils of known lengths of telegraphic wire. The results which follow are referred to a kilometre of wire, equivalent to 100 metres of Pouillet's unit (a column of mercury of 1 metre length and 1 millim. section).

The difference between the two poles of a Daniell's element, open, is represented by 100.

Electrometer.		Compass.			
			Weber's.		Ratio between two successive tangents.
Deflections.	Ratio of one deflection to the following.	Pouillet's.	1 turn of wire.	10 turns.	
146		$43\overset{\circ}{3}$ $42\overset{\prime}{}$ (tan = 0·956)	....	....	
	2·18				2·18
67·6		23 15 (tan = 0·429)	....	....	
	1·680				1·677
40·34		14 18 (tan = 0·255)	....	....	
	1·44				1·50
28		9 39 (tan = 0·17)	63·67	....	
	2·99				3
9·37		....	21·22	....	
	2·366				2·358
3·96		....	9	86·4	
	2·414				2·383
1·64				36·25	

The current being produced by eight fresh Daniell's elements, the number given by the electrometer shows in each case the total resistance of the circuit expressed in kilometres. It was $\frac{800}{146}$ in the first measurement, $\frac{800}{67\cdot6}$ in the second†.

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* This word "tension" is generally employed in the case in which the expression "potential" will be adopted.

† I suppose here that the electromotive force of a Daniell's element does not change when the current passes. Direct experiments have shown me that it is, except one per cent., the same when the circuit is open and when it is closed with as little external resistance as possible.

If from time to time we make a comparative measurement at once with an electrometer and a tangent-compass in the preceding conditions, the numbers given by the compass, multiplied by a coefficient, will show the difference between the tensions corresponding to the extremities of 1 kilometre of wire; we shall thus have the exact value of the total resistance of the circuit, if the elements employed are fresh. That value will only be approached if the electromotive force of the pile has diminished.

The use of the electrometer for the determination of the intensities of currents is known; the results obtained at different periods are comparable if we are careful to measure at the same time the difference of the tensions at the two poles of a Daniell's element.

The Daniell's element gives constant differences between its two poles when it is constituted in the following fashion:—a vessel containing a plate of amalgamated zinc and a saturated solution of sulphate of zinc; a second vessel with a saturated solution of sulphate of copper and a plate of copper: a U-tube containing sulphate of zinc, and closed at its extremities with gold-beater's skin, puts the two vessels in communication.

In the formula $i = C\dfrac{a-b}{r}$, let $C = 1$; the current having the unit of intensity will be that for which the difference of the potentials at the two extremities of the unit of resistance is equal to the unit of electric potential.

The unit of potential is the potential of a sphere, charged with the unit of electricity *, whose radius is equal to the unit of length.

If we take for the unit of intensity the intensity of the current which produces at the two extremities of 1 kilometre of telegraphic wire a difference of tension equal to one hundredth of that which exists between the two poles of a Daniell, the intensities of the currents in the above-reported experiments will be 146, 67·6, 40·34, &c.

But, in order to give more generality to the definition of the unit of intensity, it is expedient to value in electrostatic units the potential at one of the poles of a Daniell's element when the other pole is connected with the earth.

If the pole of the pile is put in communication by a long wire with a sphere of radius 1, this sphere takes the same potential as the pole of the pile, and the quantity of electricity on its surface expresses the potential of the pole.

The measurement was made with a torsion-balance, which was constructed as follows. A moveable gilt ball was fixed at the extremity of a rod of gum lac which was supported by a torsion-thread; this last was in metallic communication with the ball. A fixed ball, of the same diameter as the other, was borne by a stick of gum lac; and a fine thread connected it externally with the prolongation of the torsion-thread. The centres of the two balls were at first placed at a certain distance (about 3 centims.) without there being any torsion.

* The unit of electricity is the quantity which, acting upon an equal quantity placed at the unit of distance, viz. 1 millimetre, produces a repulsive force of 1 milligramme.

A mirror in the prolongation of the torsion-thread permitted the measurement of the displacements of the moveable ball, and, at the same time, of the torsion of the thread.

The two balls were at first connected with each other and with the earth; afterwards they were charged permanently by the negative pole of a pile of Daniell's elements, the positive pole being led to the earth.

Here are the details of an experiment, the units adopted being the millimetre and the milligramme:—

Position of equilibrium of the moveable ball (the two balls connected together and with the earth) .... } 83

The two balls communicating with the negative pole of a pile of 65 Daniell's elements .... } 178·5

The communication with the earth reestablished.... 82·75

$$178 \cdot 5 - 82 \cdot 87 = 95 \cdot 63.$$

The equation $\dfrac{q^2}{d^2} l \cos \dfrac{\alpha}{2} = n\phi$ gives the quantity q of electricity which is found on each ball.

d is the distance between the two balls. To find its value, a telescope placed at a measured distance permitted the angle to be measured under which the centres of the two balls were seen; $d = 32 \cdot 6$ millims. when the two balls communicate with the negative pole of the pile.

l, length of the gum-lac spindle, 70 millims., reckoned from the point of suspension from the torsion-thread to the centre of the ball.

n represents the force necessary to twist the thread an arc equal to the radius. This quantity is obtained by causing the thread to oscillate after replacing the stick of gum lac by a metallic spindle of known weight and length. The thread of the balance was 30 centims. long; $n = 5 \cdot 47$; with the units employed it required a force of 5·47 milligrammes, applied at the distance of 1 millim., to twist the thread an arc equal to the radius.

ϕ is the angle of torsion. The rule being divided into millimetres and placed at 3·13 metres from the mirror, $\phi = \dfrac{95 \cdot 63}{3130}$.

α, the distance-angle;

$$\frac{\alpha}{2} = 13^\circ \ 28'; \quad \cos \frac{\alpha}{2} = 0 \cdot 97.$$

We obtain $q^2 = 2 \cdot 77$, $q = 1 \cdot 76$. This is the quantity of electricity which was found upon each of the two balls. The potential will be $\dfrac{q}{r^2}$; r, the radius of one of the balls, 8·3 millims.; $\dfrac{q}{r^2} = 0 \cdot 0255$. $\dfrac{q}{r^2}$ therefore represents the density of the electricity distributed upon the sphere, taking for unit of surface in measuring the density the surface of the sphere which has the unit of length for its radius.

In this case a sphere of 1 millim. radius, communicating with the negative pole of the pile of 65 elements, is charged with a quantity of electricity which produces upon an equal mass concentrated at the

distance of 1 millim. a repulsive force of 0·0255 milligr. With a single Daniell's element, the density on the sphere of 1 millim., put in communication with the pole, would be only the sixty-fifth part of this, or 0·00039.

This number 0·00039 measures in electrostatic units the difference of potential at the two poles of a Daniell's element constituted as indicated above.

To assure myself that the balance worked properly, I ascertained, by connecting the two balls with different series of Daniell's elements, that the results found were proportional to the number of the elements—*Comptes Rendus de l'Acad. des Sciences,* Aug. 12, 1872, p. 431–435.

NOTE ON THE SPECIFIC HEAT OF HYDROGENIUM.
BY JAMES DEWAR.

The real specific heat of a substance, according to Clausius, ought not to vary with the physical state; and his calculation of the theoretical true specific heats of compound gases assumes this constant, in the case of the elementary bodies, to be identical in the free and combined state. Kopp's value of the atomic heat of combined oxygen is considerably in excess of that of the element, being 4 as compared with 2·48, whereas hydrogen is taken as 2·3, nearly agreeing with 2·4, that of the free element. As Kopp's numbers rest on the assumption of the atomic heat of a compound being equal to the sum of the individual atomic heats of the elements contained in it, and as the data are few from which the value of hydrogen has been determined, we cannot regard the mere coincidence referred to as satisfactorily proving the constancy in the combined condition. The results obtained by Kopp from calculation agree often remarkably well with experiment; and this is a strong point in their favour. It would be useful to determine the specific heat of either of the above elements in a condition approaching chemical union—that is, in the enormously condensed state in which oxygen occurs in platinum black and hydrogen in palladium. We know from Graham's beautiful researches, the result of the absorption of hydrogen by palladium is to produce a substance having all the characters of an alloy; and the specific heat of an alloy has been proved by Regnault to equal the sum of the constituents.

It is not my intention at present to describe with any detail the experiments already made in this direction; suffice it to say that, by means of a specially constructed calorimeter, the specific heat of hydrogen in palladium is found to be 3·1 per atom weight, nearly identical with that of gaseous hydrogen. The details of the experiments, and some other physical constants of hydrogenium, will shortly be given in a special paper.

XLIX. *On the Phenomena of the Elevation and Subsidence of the Surface of the Earth.* By Captain F. W. Hutton, *F.G.S., of the Geological Survey of New Zealand*[*].

IT was in 1834 that the late Mr. C. Babbage read to the Geological Society a paper on the Temple of Jupiter Serapis (Proc. Geol. Soc. vol. ii. p. 72), in which he proposed to account for the elevation and subsidence of land by the theory of the change of isothermal surfaces within the earth. Two years later, and without any knowledge of Mr. Babbage's paper, the late Sir J. Herschel also arrived at very similar conclusions (Proc. Geol. Soc. vol. ii. p. 548); and in a later communication (Proc. Geol. Soc. vol. ii. p. 596) he says that he thinks that Mitscherlich or Laplace had suggested the same theory before ; but from that time until the present no attempt has been made to follow the lead given by these distinguished philosophers, and to work out in detail the theory they started. It has, however, always appeared to me that, of all the various theories put forward to account for the elevation of land, the Herschel-Babbage theory is the only one founded upon universally acknowledged facts[†] from which results can be deduced by means of well-established laws, the accuracy of which results can be tested by observation, and the theory either proved or disproved by their

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[*] Communicated by the Author.

[†] In his lecture on Volcanoes and Earthquakes, Sir J. Herschel has quite destroyed the plausibility of his theory by saying (Lectures on Scientific Subjects, p. 11) that all he wants is a sea of liquid fire below him. He does not want it ; and few geologists would be inclined to let him have it.

means. I propose, therefore, to discuss, in as much detail as the length of a paper will allow, the bearings of this theory, and hope to show that it is adequate to account for all, or nearly all, the phenomena observed on the globe.

It will not be necessary to inquire into the much-disputed question as to the state of the interior of the earth; for whether it be solid or fluid is immaterial to the theory, provided that at no considerable depth the temperature is above the melting-point of ordinary rocks under the pressure of the atmosphere. This is universally admitted, and is proved by the small mean density of the globe and the rapid rate of increase of temperature as we descend into it. If, therefore, at some distance below the surface the rocks are heated above their melting-point, it follows that the particles composing them must be in a state of repulsion, and only kept in their position by pressure; and if that pressure is lessened at one point, the rocks will expand and the diminution of pressure will be distributed through the superheated mass until it becomes again equalized throughout. In like manner, it necessarily follows that if the pressure be increased at any point, that increase of pressure will also be distributed until the whole attains equilibrium once more. As we know that the interior heat of the earth increases at the rate of 1° F. for about 50 feet* in depth, it follows that this state of superheating will commence at a depth of about 23 miles from the surface, while above this the cohesive force will be greater than the repulsion caused by heat, and the rocks therefore will be more or less rigid, thus forming a rigid crust round a superheated or expansible interior. If, now, we suppose the interior portion to be removed, it is evident, from what we know of the strength of the materials forming the crust, that this outer portion could not support itself, but would be crushed by the enormous lateral thrust of the arch and would fall in. From these considerations it follows that the surface of the earth is maintained in its position by three forces—viz. (1) its own weight, (2) the support of the interior mass, and (3) the lateral thrust of the various portions against one another; and so long as these forces are in equilibrium, the surface will remain immoveable; but if one or other of them is sufficiently changed in intensity to overcome the rigidity of the crust, movement will follow; and movement in one place necessarily involves corresponding movements in other places, as already explained. If, for example, from some extraneous cause elevation takes place in a certain district, the pressure on the underlying rocks will be

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* The rate of increase is known to vary in different districts: but we may feel sure that it increases faster than the mean of our observations; for all borings tend to cool the rocks whose temperature we are observing.

lessened; but this reduction of pressure will spread beyond the elevated district, and will subvert the equilibrium in all surrounding areas, which will necessarily subside, and equilibrium will only be restored when the mass of the subsided portion is equal to the mass of the elevated portion. Subsidence must therefore be a necessary consequence of elevation, and *vice versâ*, unless the increase or relief of pressure is general all over the surface of the globe. A general increase of pressure is impossible, as it implies an increase in the total heat of the interior of the earth. A general decrease of pressure is doubtless brought about by the radiation of heat from the earth into space; but this decrease must have been, at any rate since tertiary times, so small that it is totally inadequate to account for the great changes that have since then taken place*. The late Mr. W. Hopkins and others have also shown that the results that we should expect to follow from a general decrease of the internal pressure do not at all accord with the phenomena we find to exist on the surface of the globe, especially in the general direction of the depressions, in the occurrence of many minor oscillations in the same place, and in the thickest or heaviest deposits having been elevated instead of depressed. Where, then, are we to look for the agencies that are at work to disturb this equilibrium, and so give rise to the movements which we know to have taken place repeatedly over many parts of the earth's surface?

There are two, and only two†, agents constantly at work to produce this effect; these are denudation and deposition. The former, as I shall presently show, has comparatively little effect; for it necessarily brings into play another force, which counteracts, or more than counteracts, the diminution of pressure caused by the removal of matter; and it is to deposition that we must look as the principal cause of all the changes that have taken place on the surface of the globe. This, as I have already stated, has been pointed out by Mr. Babbage and Sir J. Herschel; but it still remains to show its adequacy. We learn from the laws of the conduction of heat that points of equal temperature in the interior of the earth must be arranged in more or less spherical

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* Mr. Lesley has deduced that the earth ought to have shrunk 2 per cent. linearly to account for the observed geological phenomena. Upon this Mr. P. Pierce remarks (see 'Nature,' Feb. 16, 1871) that this involves a cooling of not less than 2000° C., which would melt the rocks that are supposed to have shrunk.

† Terrestrial magnetism may also have some effect; but it must be very small, as the magnetic intensity in London is only about one 6000th part of that of a single Daniell cell; and as the directions of this force are not constant, the effects that it is capable of producing will be still further reduced.

surfaces called isogeothermal surfaces, and that at great depths these isogeothermal surfaces "will be spherical; but as they approach the surface, they will by degrees conform themselves to the solid portion" (Herschel, Proc. Geol. Soc. vol. ii. p. 548) and to the mean temperature of its surface. Any change, therefore, in the temperature or outline of the surface must necessarily cause them to move; so that if a surface of rock be covered by deposits 100 feet in thickness, and the new surface have the same mean temperature as that formerly belonging to the old surface, all the isogeothermals below that area will move outwards 100 feet; and if we assume that the internal temperature increases 1° F. for 50 feet of depth, the old surface will have its temperature raised 2° F. The same result would of course follow if, instead of being covered up, the old surface had its mean temperature raised by 2° F. by a warm current passing over it. If its temperature were lowered 2° F., or if 100 feet of rock were denuded off, the effects would be just the reverse; the isogeothermals would recede 100 feet, and all the region below would be 2° colder. The effects of this increase or decrease of temperature would be to increase or decrease the volume of the rocks.

From experiments made by Colonel Totten (Lyell's Pr. of Geol. vol. ii. p. 235) and Mr. Adie (Trans. Roy. Soc. of Edinb. vol. xiii. p. 354), it appears that rocks expand from ·0000045 to ·0000095 with an accession of temperature of 1° F. If, now, the heated rock consisted of loose, incoherent particles which could move among themselves, the increase in volume would, owing to lateral resistance, all take place upward; and if we suppose the rate of expansion to be the maximum just given, we find that we should have an elevation of ·00034 inch for every foot in depth raised 1° F. in temperature[*]. This effect is so small that it would be quite imperceptible for all thicknesses of deposit and increase of temperature that we are warranted in assuming. If, however, the particles of which the rock was composed were rigidly fixed together, the case would be very different; the expansion in height would still take place upward; but those in length and breadth could not be so diverted, and they would give rise to an irresistible pressure, which could only be relieved by the rock rising up in one or more anticlinal ridges. Now sands and clays are always deposited in the incoherent state first mentioned; but deposits of carbonate of lime are indurated almost immedi-

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[*] It must be remembered that in this case the lateral thrust would not have to support the arch entirely. As soon as sufficient lateral thrust was developed to overcome the rigidity of the crust, movement would commence, and the remainder of the force necessary for supporting the arch would be borne by the expansible underlying rocks.

ately after deposition, and therefore the expansion which must necessarily follow the formation of thick deposits would throw it into anticlinal ridges. The same would occur with incoherent rocks when they were covered up with limestone—with this difference, however, that the amount of lateral pressure developed would be limited by the weight of the superincumbent deposits. The deposition of limestone, therefore, relieves the pressure, while the deposition of sand and clay increases it.

The following Table exhibits approximately the height, in feet, of the anticlinals caused by an accession of heat to rocks forming the surface of a sphere with a radius of 3956 miles, the rock being supposed to expand in length ·000005 for each degree of temperature. The upper line shows the thickness, in feet, of the deposit necessary to raise the temperature the number of degrees indicated in the second line from the top. The left-hand column shows the breadth of the anticlinal in miles.

Thick.	500 ft.	1000	1500	2000	2500	5000	10,000	15,000	20,000	25,000
Temp.	10°.	20°.	30°.	40°.	50°.	100°.	200°.	300°.	400°.	500°.
Dist. miles.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.	feet.
100	1140	1950	2550	3100	3700	5,650	8,700	10,900	12,700	14,600
200	1200	2600	3700	4650	5500	9,200	14,700	19,300	22,700	25,100
300	1450	2850	4250	5300	6350	11,400	19,100	25,400	33,100	36,800
400	1530	3000	4350	5800	7050	12,860	21,650	30,160	37,470	43,400
500	1550	3050	4500	5900	7220	13,200	24,200	33,400	41,550	49,300
1000	1570	3080	4650	6150	7700	14,500	28,600	41,700	53,750	65,400
2000	1900	3200	4700	6250	7800	15,400	30,700	45,600	57,300	74,400

It appears from this Table that deposits no thicker than we know some formations to be, spread over areas of moderate extent, are capable, under favourable circumstances, of producing higher mountains than any we know at present; but it will be noticed that this operation will only take place on an extensive scale if the rate of heating from below is slower than the rate of deposition from above; for if such were not the case, the elevation would follow so quickly after the formation of the limestone that no very thick deposit could be formed before the bed was raised to the sea-level. If, however, deposition took place more rapidly than the conduction of the heat outward, large deposits would be formed, which would be below the normal temperature due to their depth from the surface; and these therefore would continue to expand, and therefore to rise, until their normal temperature was attained; and the greater the difference between the rates of deposition and conduction, the greater would be their ultimate height. It is therefore necessary to examine this point.

Baron Fourier has calculated that the earth decreases in temperature 1° F. in 3,000,000 years. Now a hollow shell one mile thick surrounding the earth is to the rest as 1 : 1316; if, therefore, this shell collected all the heat radiated outward, it would be raised in temperature 1316° F. in 3,000,000 years. If, now, we suppose the total amount of this heat to be spread out through a shell of rock in such a manner that it decreased outward $\frac{1}{50}$° F. for every foot until it was zero on the outside, a simple calculation will show that this shell would have to be 26,364 feet in thickness; so that if the earth were enclosed in a shell of this thickness, the internal heat would reach the surface in 3,000,000 years; consequently it would have travelled outward at the rate of ·1 inch per year. Sir W. Thomson (Proc. Roy. Soc. of Edinb. 1864, p. 200, and 1865, p. 512), basing his calculations on the experiments on the conductivity of rocks made by Forbes, Glaisher, Ångström, and Peclet, concludes that the earth decreases in temperature 1° C. in 2,000,000 years, or 2°·7 F. in 3,000,000, which gives an outward conduction of ·27 inch per year. These experiments, however, were made on dry rocks, while newly formed deposits would be saturated with water; and if we suppose that the thermal capacity of mud and sand saturated with water is the same as that of water, his calculations will have to be reduced by one half, which would make the outward conduction ·135 inch per year. Peclet's experiments show that the conductivity of marble is only two fifths of the average taken by Sir W. Thomson; so that in limestone the conduction outwards might only be ·1 inch per year. These calculations, therefore, although founded on but few experiments, agree so closely with the results arrived at by Baron Fourier, that we may place considerable confidence in their accuracy; and if we assume that the internal heat is conducted outward through limestone at the rate of ·1 inch per year, and through wet sand and mud at the rate of ·13 inch per year, we shall probably be not very far from the truth.

Prof. Dana (Manual of Geology, p. 386) estimates that limestone grows at the rate of ·125 inch per year, and sedimentary rocks from five to ten times as fast, or from ·62 to 1·25 inch per year. The Mississippi delta has been estimated to increase in thickness ·17 inch per year, and that of the Ganges ·26 inch per year. The mean of these estimates is ·6 per year; and if we assume that in a formation one third consists of limestone, the average rate for the whole would be ·4 inch per annum; so that the rate of deposition would be more than three times as fast as the rate of heating.

From this it follows that when the internal temperature increased three times as fast as it does now, or at the rate of $\frac{1}{17}$° F.

for each foot, no land would rise from this cause above the sea-level; for as the temperature would then increase as fast as the deposition, as soon as the latter stopped the former would stop also. This, according to Sir W. Thomson's calculations ("On the Secular Cooling of the Earth," Trans. Roy. Soc. Edin. 1862), would have happened about 11,500,000 years after the cooling of the crust, or some 88,500,000 years ago; and from that time to the present, elevations must have been on an increasing scale.

If also the interior heat travels outward at from ·13 to ·1 inch per year, it would take from 46,200 to 60,000 years to advance 500 feet, which is equal to an increase of temperature of 10° F. Now from the Table we find that an increase of temperature of 10° F. implies an elevation of 1140 feet if the heated area was 100 miles in diameter, or of 1900 feet if its diameter was 2000 miles; so that in the first case an elevation of 1140 feet would have taken place in from 46,200 to 60,000 years, or at the rate of from 1·9 to 2·44 feet per century, while in the latter case the elevation would have been 1900 feet in the same time, or at the rate of from 3·17 to 4·11 feet per century. We may therefore conclude that elevation from this cause proceeds at a rate of from 2 to 4 feet per century.

Professor Geikie and Mr. Croll have shown that subaërial denudation is a very slow process, requiring from 700 to 12,000 years to remove a foot in thickness, so that at present it will hardly affect the elevation; but as the earth cools, the radiation and conduction outwards will be slower, and consequently elevation will be slower also; and a time will come when it will not exceed the rate of denudation, and when consequently no land can be formed, and the earth will again relapse into a quiescent globe surrounded by water[*]. If we take the least estimate of denudation, or 1 foot in 12,000 years, as that which will be nearest to the truth when the land is nearly all flat, it follows that the elevation, if it just keeps pace with the denudation, will have to be only $\frac{1}{360}$ of its present rate, or the interval of temperature must increase only at the rate of $\frac{1}{18000}$° F. per foot. This, according to Sir W. Thomson's theory, will not take place until about 13 billions of years have passed; consequently the earth is as yet only in its infancy between birth and the repose of old age.

Before proceeding any further with these speculations, it will, I think, be advisable to give an illustration of this theory taken from nature.

In his celebrated paper on the structure of the Wealden

[*] Unless indeed the oxidation of the interior had before this time absorbed the water and the oxygen from the air, thus leaving a bald earth surrounded by a thin atmosphere of nitrogen, which is not likely.

district (Trans. Geol. Soc. Ser. 2, vol. vii.), Mr. W. Hopkins showed that if a district is elevated by the upward pressure of an underlying fluid mass, tension will be produced in the rocks, which will cause the formation of two systems of fissures—one parallel to the major axis of the area undergoing elevation, the other at right angles to its periphery; and he takes the Wealden district as an example agreeing in all respects with his deductions, thus proving that the Weald anticlinal has been formed in this manner. Now, without for a moment questioning the effects which Mr. Hopkins has shown must necessarily follow from such an upward pressure, it is quite allowable to inquire whether he has been happy in selecting the Wealden area as an illustration, and whether the Herschel-Babbage theory will not give a far better explanation of the phenomena. In his able description of the district, Mr. Hopkins represents the chalk escarpment which surrounds it, and which is supposed to be parallel with the periphery of the elevating fluid, as being traversed by transverse faults or fissures which have originated the valleys in which the rivers now flow that drain the area of the Weald, these fissures being those of his second system. He also describes longitudinal minor anticlinal axes, or "lines of elevation," as he calls them, parallel to the major axis of the elevated district; and these, curiously enough, he considers as answering to his first system of fissures. Mr. Hopkins candidly acknowledges that one only of the transverse fissures of his second system, that between Battle and Hastings, rests on positive evidence, all the others are only supposed to exist owing to the direction of the valleys. I need hardly say that at the present time, with our greater knowledge of surface-geology, this evidence is quite worthless; and I feel sure that Mr. Hopkins would not now bring it forward in support of a theory. On the other hand, the longitudinal "lines of elevation" are observed facts; but they are nearly all anticlinal and synclinal folds, and indicate compression and not tension. The evidence, therefore, goes to show that this area has not undergone tension, and that therefore it has not been raised by the upward pressure of an underlying fluid mass.

If, now, we attempt an explanation of the structure of this district by means of the Herschel-Babbage theory, we have for our data a thickness of 2100 feet for the Cretaceous formation, and 1300 for the Wealden formation, making a total thickness of 3400 feet. The Wealden beds on the anticlinal reach a height of some 800 feet above the sea-level; and to this we must add the thickness of the removed deposits, thus making the original height about 3600 feet above the sea. At London the chalk is about 500 feet below the sea-level; so that

the total rise of the arch was about 4100 feet, while its breadth from London to some point in the English Channel may be taken at 100 miles. If now we turn to the Table, we find, by interpolation, that a thickness of 3400 feet implies a rise in temperature of 68°, which, acting over a breadth of 100 miles, would cause an elevation of about 4650 feet. An ample margin is therefore allowed for the check to the upward movement of the isogeothermals caused by denudation after emerging from the sea. The lower beds being more highly heated than the upper chalk, would expand more and give rise to those minor folds, or "lines of elevation," described by Mr. Hopkins.

Another illustration may be desirable. During the early eocene period an extensive ocean existed from Spain and Morocco eastward through Switzerland, North Africa, Turkey, Asia Minor, Persia, and Northern India to China; and in this ocean the nummulitic limestone, some 8000 feet thick, was deposited. Here, therefore, if there is any truth in the theory that thick and extensive limestone deposits cause elevation, we ought to find marked evidence of it; and accordingly in the Atlas, Pyrenees, Alps, Apennines, Carpathians, Persian mountains, and Himalaya, all of which have been elevated since the nummulitic limestone was deposited, we see effects produced commensurate in their grandeur with the extent and thickness of the formation. For if the deposition of the nummulitic limestone and the overlying deposits was not the cause of the elevation of these mountain-ranges, we have to seek for some other cause acting over exactly the same area at the same or nearly the same time, and yet, if each chain had its own independent origin, acting in different directions. It certainly seems to me far more reasonable to view them simply as wrinkles in the limestone caused by its expansion; for it must be remembered that by this theory mountain-chains would be formed at those places where the deposits were thickest, so that they might take various directions although all formed at the same time. It is also remarkable that the highest mountains, although situated in various parts of the world, have all risen to about the same height (8000 feet) above the snow-line. This, although difficult to account for in any other way, is readily explained by supposing that the cold of the upper regions has stopped their growth by preventing the further outward movement of the isogeothermals. In this way perhaps we may account for the Himalaya being higher than the Alps, without having to suppose that the nummulitic formation was thicker in India than in Switzerland.

I have already pointed out that an alteration in the mean temperature of the surface will produce the same effects as the deposition or removal of matter; and in this way many of the

movements of the surface can be accounted for. For example, the Gulf-stream carrying a high temperature into northern latitudes may be the cause of the recent elevation of Sweden, while the cold arctic current sweeping down through Davis's Straits may be the cause of the depression of Greenland[*]; for on a spherical surface contraction would cause depression by decreasing the lateral thrust and throwing more weight on to the underlying superheated rocks. Again, if, as explained by Mr. Croll, the earth should enter upon a cold period, all land of a higher mean temperature than $32°$ would have a tendency to sink, while that previously covered by snow or ice would not be affected, thus helping to increase the cold. The submergence of a large part of North Wales during the glacial period, and its subsequent reemergence, may perhaps have been owing to the cold of the period itself, followed by a return to its original temperature. A difference in temperature of about $10°$ would be sufficient to effect this. South Sweden was also depressed when the cold was most severe, and has since been raised more than 200 feet.

I will now proceed to consider the second cause of oscillations, viz. the alteration of pressure by the denudation and deposition of rocks. In this case, unfortunately, I cannot make even approximate calculations as to the weight of deposits necessary to cause movement, in consequence of our ignorance of the rigidity of the outer crust of the earth. We certainly know that this rigidity is sufficient to overcome the attraction of the moon; but as this force acts only for very short periods in the same direction, the inertia of the rocks will help to supplement the rigidity; so that I do not see how even a minimum quantity can be obtained for it. Every geologist, however, knows many facts proving that all thick formations have been deposited during subsidence; and the fossils in these formations often prove that the subsidence has been about equal in rate to the deposition, and also that in most (if not all) cases high land has been not very far off. Now it is highly improbable that the rate of deposition and subsidence should coincide so often unless one was caused by the other; and as we know from *à priori* reasoning the great probability there is of an increase of pressure causing subsidence, we may, I think, safely assume that such is the case. What, then, would be the effects resulting from this cause? Where denudation was going on, the mass of land would be lessened; but, as already stated, the rate of denudation is much slower than the conduction of heat through rocks, consequently the isogeothermals would recede as fast as the denudation went

[*] The climate of Greenland is known to have decreased in temperature during the last 900 years. See 'The Land of Desolation,' by I. J. Hayes, M.D. London, 1871.

on, and the contraction of the rocks from this cause would more than counterbalance the relief of pressure, and subsidence instead of elevation would be the result. Where deposition was going on, the isogeothermals would be rising; but as deposition is more rapid than the outward conduction of heat, the underlying rocks would not be able to uplift the incumbent mass, which being loose sand and mud would render no assistance, and the area would sink. The underlying rocks, therefore, as well as the lower parts of the new formation, would be undergoing compression from forced subsidence, and at the same time expansion from an increase of heat, and foldings and contortions would be the consequence.

If, now, we assume, what the facts tend to show, that subsidence equals deposition, we can calculate the amount of compression the underlying rocks will undergo from subsidence, and the amount of their expansion from the increase of heat due to the thickness of the new deposits, the two together being the amount of squeezing available for contortions. The following Table gives approximately the sum of these two in decimal parts of a mile for areas of 100, 200, and 300 miles breadth. The upper line gives the thickness of the formation in feet, which is supposed to be equal to the depth of the subsidence :—

Miles.	1000 feet.	5000 feet.	10,000 feet.	15,000 feet.	20,000 feet.	25,000 feet.
100	·014	·095	·23	·415		
200	·028	·15	·33	·5	·7	1
300	·04	·21	·42	·675	·9	1·5

From this Table we can deduce the following, which will be sufficiently accurate for practical purposes in testing the theory:—

Thickness of beds, in feet...	1000	5000	10,000	15,000	20,000	25,000
Amount of compression......	$\frac{1}{7000}$	$\frac{1}{1000}$	$\frac{1}{500}$	$\frac{1}{400}$	$\frac{1}{300}$	$\frac{1}{200}$

These results may be thought to be much too small to account for the great contortions and foldings that we see in mountainous districts; but it must be remembered that these districts have been elevated and depressed many times, as proved by the unconformities among the strata, and that the contortions now observed will be the sum total of all these movements. In the section given by Professor A. Ramsay of North Wales, from Snowdon to Aran Mowddwy (Mem. Geol. Surv. of Great Britain, vol. iii.), I find that, after making due allowance for faults and dykes, the compression of the rocks has been $\frac{1}{16}$. But even in this section, admirable as it is, the data are not sufficient for comparing the

observed facts with theoretical deductions; for in order to do this we must know the thickness of all the formations that have ever overlain the district, and the number and amount of the oscillations they have undergone; and even then there might be various causes, such as alterations in temperature, which would affect the result.

I have now explained the Herschel-Babbage theory in its simplest form; but in nature this simplicity would seldom or never exist. Complications would arise (1) from changes in physical geography causing changes in the surface-temperature, according to the theory of Sir C. Lyell, (2) from the movements of the isogeothermals being often oblique, (3) from the different degrees of fusibility of rocks, (4) their different conducting-power and (5) rates of expansion, and (6) their varying degrees of porosity, and also (7) from new fluctuations of temperature commencing before the old ones had terminated. The two great motive powers, alteration in volume and increase of weight, would also sometimes combine, and at other times interfere with one another like cross waves on the surface of the sea, and would thus give rise to the great irregularities that we see in nature.

I will now shortly consider from the stand-point given by this theory some of the principal geological phenomena that have not yet been mentioned.

Volcanoes require an upward pressure of the superheated rocks and sufficient tension in the upper rigid crust to form fissures; they ought therefore to be situated on land rising from the upward pressure derived from adjacent subsiding areas, which are sinking either from an increase of pressure or from a decrease in temperature. This can certainly be shown to be the case with the volcanoes of South America, the Pacific*, Indian Archipelago, and Iceland; but it is not so easy to account for Etna and Vesuvius.

Faults can only be formed when the rocks are undergoing tension, as it is mechanically impossible that compression should force up a wedge of rock; for it would crush or bend before it would move. After a district has been folded by compression the rocks would not, on tension following, go back into their former position, but fissures would be formed through them, and those wedge-like masses that have their point turned downwards would descend by their own weight; or if the land be stretched by pressure from below, those with their points turned upwards will be driven upwards.

Cleavage.—Mr. Sharpe has remarked (Quart. Journ. Geol. Soc. vol. iii. p. 104) that "there are reasons for thinking that

* Coral islands would not cause elevation, as they are not connected with one another.

pressure could not have been the sole agent in the operation; for the cleavage did not take place on the first upheaval of the district, when, the crust not having yet given way, the pressure might be supposed the greatest, but only after the beds had assumed their present position and the various anticlinal axes had been formed." The Herschel-Babbage theory, however, entirely gets over this difficulty; for the pressure would not be so great during the upheaval caused by limestone, as afterwards when the refrigeration and contraction consequent on denudation had come into play; for the extra weight thus thrown on to the underlying superheated rocks would give rise to an upward pressure which would force dykes into the overlying beds, and give rise to a cleavage that would either obliterate former ones, or intensify them where the planes of the two coincided.

Conclusion.—If the surface of the earth were level and the sea spread evenly over it, the depth of this universal ocean would be at least two miles; and as we cannot suppose that in a sphere slowly cooling from an incandescent state any gases or other volatile substances would have been retained in the interior so as to produce eructations on the surface, the question naturally arises, what could first have caused the subversion of the equilibrium that has ultimately led to such stupendous changes? There can only, I think, be one answer to this question, viz. the origin of life on the globe. This life, by abstracting the carbonate of lime from solution in the sea* and depositing it on the bottom first disturbed the equilibrium, and prepared the way for the countless multitude of forms that now crowd over the surface of the globe†. When the earth was entirely enveloped by the ocean, the trade-winds would cause surface-currents flowing in N.E. and S.E., and undercurrents flowing in N.W. and S.W. directions; and if we suppose life to have originated at any one spot these currents would spread the organisms in these directions, and the first deposits, and consequently the strike of the first-elevated rocks, would have these directions also. The direction of the first land, by affecting the distribution of future deposits, would probably exert an influence even up to

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* Bischof (Chemical Geology, vol. iii. p. 37) and Sterry Hunt (American Journal of Science [2] vol. xxxix. p. 184) both agree that the primeval ocean probably contained carbonate of lime in solution.

† It follows from this that it is to the ocean that we must look for the nearest representatives of primordial life. The freshwater monads and bacteria are probably quite complicated beings compared with the first-formed organisms. It may be that the organic matter distributed through the ocean at great depths, and which is so small as to be invisible to our most powerful microscopes, and only capable of being recognized by chemical reagents, is composed of living organisms intermediate in structure between the Rhizopoda and the first living germs.

the present time, but getting more and more irregular; and I need hardly say how well this agrees with the observed facts both of the present disposition of mountain-chains, and the strike of the oldest known Laurentian rocks.

Summary.—Mountain-chains may be divided into two classes— the one characterized by folds and contortions associated with metamorphic rocks (such as the Alps), the other by slightly inclined beds associated with volcanic rocks (such as the Andes). The former class of mountains are owing to heavy masses of clay and sand having caused subsidence and contortion, and then having been subsequently elevated by the superposition of a thick bed of limestone. The latter class are owing to the depressions caused by deposition necessitating an equal uprising in other places. Many intermediate varieties also occur, which are owing partly to one cause and partly to the other. Such is a sketch of the Herschel-Babbage theory; to elaborate it in detail would require a far greater knowledge of the geology of the world than I possess. But although it may be shown that it is not in itself sufficient to account for all observed phenomena, yet I hope that some good will arise to science by sifting out by its means from our heterogeneous mass of facts all those that it will explain, and thus limiting the residual phenomena in a way that will be most likely to lead ultimately to a complete solution of the question.

L. *On the Relation which the internal Resistance of the Battery and the Conductivity of the Wire bear to the maximum Magnetizing Force of an Electromagnet Coil. By* G. K. WINTER, *F.R.A.S., Telegraph Engineer, Madras Railway**.

IN Poggendorff's *Annalen* for November 1865, and in the Philosophical Magazine for December of the same year, is published an investigation by Dr. Menzzer on the relation of the weight of a magnetizing spiral to the magnetizing force. In this paper the author shows that, *cæteris paribus*, the magnetizing powers of two coils, arranged to give the maximum force with a given battery, are as the square roots of their respective weights. In this investigation the average length of a convolution is supposed to be constant; and as the iron rod to be magnetized is, I presume, supposed to remain the same, the length of the helix will also be a constant. It is difficult to conceive, under these circumstances, how the law can be applied, except within narrow limits—since, the size of the coil being constant, the weight of the wire must be a constant too,

if we except a slight variation due to the different proportion of the space occupied by the insulating material according to the size of the wire.

In looking into this subject a short time since, I found two other relations regarding electromagnet spirals or coils, equally simple in their nature to that we have noticed, but not subject to a similar anomaly. I believe these relations are new; and they are certainly interesting, if not of any very great practical value.

For the sake of clearness and brevity, it will be advisable to give here two definitions of terms it is proposed to make use of.

1. The minimum resistance of a battery is the resistance it would have if all the positive and all the negative poles were respectively joined together so as to form one element of large surface.

2. The reduced resistance of a given wire is the resistance of a wire of uniform gauge, equal in volume and specific conductivity to that of the given wire, and one metre in length.

Let

r = the minimum resistance of the battery to be employed,

n = the number of elements to be joined in series,

e = the electromotive force of one element,

R = the reduced resistance of the wire forming the magnetizing coil,

l = the length of the wire,

C = the strength of the current,

k = the conductivity of the wire,

M = the maximum magnetizing force of a given combination;

then

$$C = \frac{ne}{rn^2 + Rl^2}. \qquad \cdots \cdots \quad (1)$$

The condition of maximum effect is

$$rn^2 = Rl^2.$$

Therefore

$$l = n\sqrt{\frac{r}{R}}, \qquad \cdots \cdots \quad (2)$$

and

$$C = \frac{ne}{2rn^2} = \frac{e}{2rn}. \qquad \cdots \cdots \quad (3)$$

The size of the frame on which the wire is wound being constant, the number of convolutions will, of course, be proportional

to the length of the wire; we may therefore say

$$M = \frac{le}{2rn} = \frac{n\sqrt{\frac{r}{R}}e}{2rn} = \frac{\sqrt{\frac{r}{R}}}{2r} = \frac{e}{2\sqrt{Rr}}. \quad (4)$$

The maximum magnetizing force, therefore, varies directly as the electromotive force of one element of the battery, and inversely as the square root of the product of the reduced resistance of the coil-wire into the minimum battery resistance.

The volume of the wire used in the coil being constant, its reduced resistance will vary inversely as its conductivity, or

$$k = \frac{1}{R}.$$

Then

$$M = e\frac{\sqrt{k}}{2\sqrt{r}}. \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (5)$$

Supposing the dimensions of the helix to remain constant, and the combination to be always arranged to give the maximum effect, we learn from this equation :—

1st. *That, the battery remaining the same, the magnetizing force will vary directly as the square root of the conductivity of the wire.*

2ndly. *That, the conductivity of the wire remaining constant, the magnetizing force will vary inversely as the square root of the minimum battery resistance; or since, if the battery consist of a number of cells, equal in all respects, the minimum battery resistance will be inversely as the number of cells employed, the magnetizing force will vary directly as the square root of that number.*

It must be remembered that we are dealing, in the above relations, with the magnetizing powers of the helices, and not with the amount of magnetism developed by them; this magnetism, however, according to the laws of Lenz and Jacobi, varies as the magnetizing force within wide limits—that is, so long as we are not approaching too near to the maximum magnetic force the iron is capable of receiving.

While we are upon this subject, it might perhaps be well to point out a very useful application of equation (5) in my paper published in the Philosophical Magazine for February 1870, page 113.

We have frequently to wind a coil of given size with wire, and at the same time it is necessary that the coil should have some given resistance; the question arises, what must be the size of the wire? A slight modification of the equation above mentioned will enable us to answer this question without difficulty.

The equation is as follows:—

$$G = \frac{\cdot0000002\, lm}{ar^2(r+s)^2c},$$

in which

$c =$ the specific conductivity of the wire, that of pure copper at $0°$ C. being unity,

$l =$ the average length of a convolution in inches,

$m =$ area in square inches of a semisection of the coil (that is, the sectional area of the space to be filled with wire),

$s =$ thickness, in inches, of the insulating covering,

$G =$ the resistance of the coil, in ohms,

$r =$ the radius of the wire, in inches,

$a =$ a constant depending on the method of coiling the wire, but generally $= 4$.

Let

$\gamma = \cdot0000001$;

$d =$ the diameter required, in inches.

Then

$$G = \frac{\gamma lm}{2r^2(r+s)^2c}, \quad \cdots \cdots \quad (6)$$

$$r^2(r+s)^2 = \frac{\gamma lm}{2Gc}$$

$$r = \sqrt{\sqrt{\frac{\gamma lm}{2Gc} + \frac{s^2}{4}} - \frac{s}{2}}. \quad \cdots \quad (7)$$

As s is generally very small, we may write this equation

$$r = \sqrt[4]{\frac{\gamma lm}{2Gc}} - \frac{s}{2}, \quad \cdots \cdots \quad (8)$$

and $\therefore$

$$d = 2\sqrt[4]{\frac{\gamma lm}{2Gc}} - s. \quad \cdots \cdots \quad (9)$$

LI. *On the Spectroscopic Reversion-Telescope.*
By F. Zöllner*.

[With a Plate.]

A YEAR since, I had the honour to communicate to the Royal Society of Saxony the successful application of my reversion-spectroscope to the observation of the rotation of the sun†. In endeavouring to introduce the extraordinarily delicate principle

which underlies this instrument, viz. the principle of *the rever-sion of spectra,* to general use in the determination of the posi-tions of the lines in spectral analysis, I have striven to remove the circumstances which permit to the reversion-spectroscope, constructed three years since*, only a limited applicability. The principal of these circumstances were the following :—first, that it was absolutely necessary to employ prisms with direct vision ; and secondly, that the relative displacement of the two spectra could only be effected by means of the displacement of the halves of the object-glass of the telescope. In both these ways was the applicability of the principle confined to a certain very limited portion of the spectrum. The generally immove-able attachment of the illuminating object (the slit) to the ob-serving-telescope, however, has enabled us to effect the reversal of either the entire or divided spectrum simply by means of a reversing-prism with total reflection. By this the relative dis-placement of the two spectra is at the same time connected with the variations of the angle of the plane of reflection to the optic axis of the collimator ; so that the mobility of the two halves of the objective parallel to the plane of division is no longer necessary. The alterations of angle of the face of the total-reflection prism can be effected in two ways—either by micrometric motion of the prism alone, or by moving the observing-telescope in combi-nation with the prism. It would in general be advantageous to combine the two motions,—the first, *micrometric,* for differential determinations, the second for determinations of position of *all* the lines of the spectrum with the aid of an index and a graduated arc.

The reflecting prism can have two different positions in the observing-telescope, viz. either with the object-glass or the eye-piece.

The advantages, corresponding to the particular purpose, ob-tained by the use of the one or the other construction I have discussed *l. c.*

You will permit me now to lay before you a perfectly finished specimen of a spectroscopic telescope with a reversion-objective, and at the same time some measurements which have been ob-tained with a reversion-eyepiece in another telescope, executed by M. Merz from data supplied by me.

The telescope with reversion-objective is represented in fig. 1 (Plate IV.), one third of the natural size.

The telescope A contains at B the objective-halves moveable perpendicularly to the plane of division. By this motion we can either cause one of the spectra to overlap the other, or make

* *Berichte der K. Sächs. Ges. der Wiss.* Feb. 6, 1869. Phil. Mag. November 1869, p. 360.

Fig. 1.
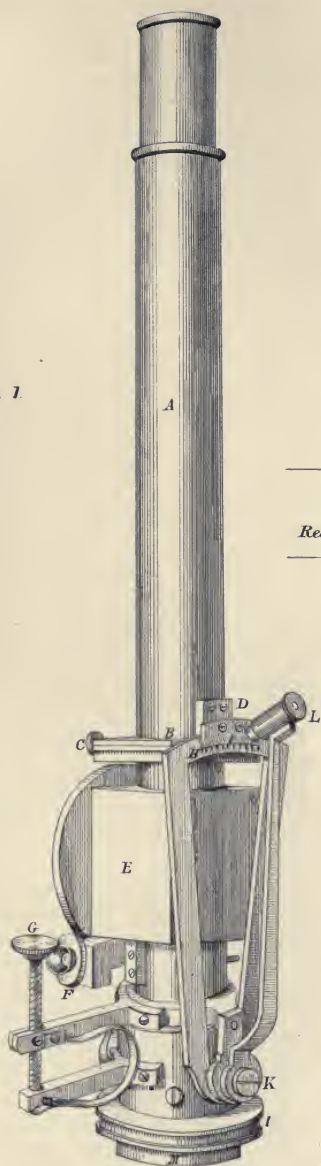
Fig. 2.
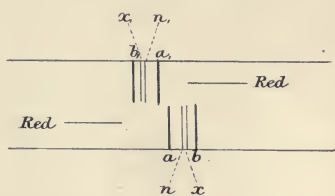
them appear distinctly side by side like a vernier and scale. For this purpose we have only to move the arms of the objective more or less from one another, and not, as before, to shift them micrometrically parallel to the plane of division after the manner of the heliometer.

The reflection-prism is placed in the receiver E, and is moveable in front of the halves of the objective by means of the screw F. The screw G permits the telescope to be moved round the axis K, and thereby effects, as remarked above, the relative displacement of the two spectra. The magnitude of this angular motion can be read off on a graduated arc H with the aid of the lens L.

At I the rays from the system of prisms enter the telescope. By means of the screw-thread M the instrument can be combined with any spectral apparatus; and for the purpose of adjusting the refracting edges of the dispersing and reflecting prism (E), it can be turned upon its longitudinal axis by means of the annular appendage I.

The Merz spectroscope contains two direct-vision prism-systems, which can be used either singly or combined. As in the usual arrangement of that kind of spectroscope the collimator is at all times immoveably fixed to the prism-piece, the strong dispersion of the prisms used limits the observation to a proportionally small portion of the spectrum. I therefore proposed to M. Merz to remedy this inconvenience by making the collimator-telescope in like manner moveable and its position controllable by an index and graduated arc. This alteration, which I had previously had applied to the spectroscopes manufactured here, answered the purpose completely. It is obvious that then the graduated arc on the collimator served not to measure, but merely to mark the best position of the collimator for a certain part of the spectrum.

The magnitude of the dispersion and the clearness of the images produced by the spectroscope made for me and recently sent to me from Munich are so considerable that, between the two sodium lines in the solar spectrum, besides the nickel line, there is seen distinctly a finer and more refrangible line, and that, too, when the sun is at his greatest altitude.

This instrument is converted into a reversion-spectroscope by merely substituting for its eyepiece-cover one containing a small reflection-prism which covers exactly half of the aperture. The reflecting face stands parallel to the optic axis of the instrument, and thus, being at the same time parallel to the refracting edges of the dispersion-prisms, effects a partial inversion of the spectrum. When the instrument with such a reversion-eyepiece is directed to a candle-flame impregnated with sodium, two partially overlapping spectra are seen, which move in opposite di-

rections when the observing-telescope is moved by the micrometric screw, and afford an extraordinarily accurate observation of the coincidence of homologous lines. In order to effect this partial overlapping of the two spectra in observations of the dark lines of the solar spectrum, it is necessary to cause divergent rays likewise to fall upon the slit. Hence, if the spectroscope is not combined with a telescope (in which case coincidence of the plane of the slit with the optical image of the sun satisfies the above-mentioned condition), a small lens of short focal distance fixed in front of the slit imparts to the rays the requisite property.

Fig. 2 represents the impression made by the two sodium lines when the most powerful eyepiece in the described spectroscope is employed; n is the nickel line, and x the above-mentioned fainter line.

In order to give an idea of the great accuracy attainable in the determination of the positions of lines by the employment of the reversion-prism, I take the liberty to add a number of measurements of the distance of the sodium lines and of the others found between them. The numbers given are parts of the circumference of the screw; on which it is to be remarked that for these measurements the thread was not sufficiently fine, as the tenth part of the values given had to be estimated. The letters placed together at the head of each column denote the lines which were brought to coincidence in the measurements.

Series 1.				Series 2.			
aa_1.	bb_1.	bb_1-aa_1.	$\frac{1}{2}(bb_1-aa_1)$.	aa_1.	an_1.	ax_1.	ab_1.
25·7	31·6	5·9	2·95	26·0	27·4	28·0	29·0
25·7	31·7	6·0	3·00	26·0	27·5	28·0	29·1
25·8	31·7	5·9	2·95	26·0	27·6	28·1	29·0
25·9	31·8	5·9	2·95	26·1	27·6	28·1	29·1
25·9	31·8	5·9	2·95	26·1	27·6	28·1	29·1
26·0	31·8	5·8	2·90	26·2	27·5	28·1	29·1
25·9	31·8	5·9	2·95	26·2	27·6	28·2	29·0
25·8	31·9	6·1	3·05	26·2	27·7	28·1	29·1
26·0	31·9	5·9	2·95	26·1	27·5	28·2	29·1
25·9	31·9	6·0	3·00	26·2	27·5	28·1	29·0
			Mean 2·965 + 0·009	26·11	27·55	28·10	29·06

It will be seen that the delicacy of the measurements is extraordinarily great, and well justifies the hope that, with the speedy improvement of spectroscopic instruments, my endeavours to demonstrate the rotation of the earth from the displacement of the lines in the solar spectrum will before very long be successful, in like manner as the rotation of the sun has been already shown

satisfactorily by Vogel's observations[*]. For at the rising of the sun a point in the equator moves toward it with a velocity of about $\frac{1}{19}$ of a geographical mile[†], and at sunset with about the same velocity from it, so that in the course of twenty-four hours the point undergoes, in consequence of the rotation, a variation of about $\frac{1}{10}$ of a mile in the velocity of its motion relative to the sun. But this quantity would alter the position of the sodium lines $\frac{1}{400}$ of their distance, and hence, with the aid of the reversion-prism, amount to $\frac{1}{200}$ of the ascertained distance. Now, since the mean value derived above from ten observations shows a probable error of only $\frac{1}{329}$ of the said distance, it is evident from this how near we have already approached to the successful solution of the problem. How great an importance these observations may acquire in regard to ascertaining both the velocity of light and, through the connexion of this with the constant of aberration, the parallax of the sun, cannot *à priori* be estimated, depending entirely on the progressive improvement of spectroscopic instruments.

Relative to the facility of the general application of the reversion principle in spectrometric investigations, I take leave, in conclusion, to state that I have succeeded, by a combination of two reflection-prisms immediately behind the eyepiece-cover, in constructing a reversion eyepiece which perfectly fulfils the condition of exact superposition of the two spectra. The reflecting hypotenuse surfaces of the prisms are simply placed perpendicular to each other, one of them being parallel to the width, and the other parallel to the length of the spectrum. One half of the eyepiece-aperture is supplied with light from the first, the other with light from the second prism; so that the first prism effects the *reversion* of the spectrum, while the other, with corresponding inclination of the plane of reflection, accomplishes the *juxtaposition* of the, as regards the order of the colours, unchanged spectrum. The action of this eyepiece is surprising: for example, when combined with a Browning's miniature spectroscope it is at once converted into a measuring-apparatus which, by micrometric motion of the reversion-prism, permits the determination of the positions of the lines by coincidence with all the delicacy desirable in so compendious an instrument.

As soon as a sufficient number of measurements have been accomplished according to this method upon star-spectra, I will take leave to communicate to the Royal Society the results of my observations.

* Conf. *Ber. d. K. Sächs. Ges.* July 1, 1871; Phil. Mag. S. 4. vol. xliii. p. 47; and 'Beobachtungen angestellt auf der Sternwarte des Kammerhern *von Bülow* zu *Bothkamp* von Dr. *H. C. Vogel*, Astronom der Sternwarte.' Leipzig (*Engelmann*), 1872.
† 1 German = 4 English geographical miles.

LII. *Researches in Actino-Chemistry.*—Memoir Second. *On the Distribution of Chemical Force in the Spectrum. By* John William Draper, *M.D., LL.D., President of the Faculties of Science and Medicine in the University of New York*[*].

WITH scarcely an exception, the most recent works on the chemical action of radiations and spectrum-analysis describe a tripartite arrangement of the spectrum, illustrated by an engraving of three curves, exhibiting the supposed relations of the calorific, the luminous, and the chemical spectra. This view, which by a mass of evidence may be shown to be erroneous, is exerting a very prejudicial effect on the progress of actinochemistry.

I propose now to present certain facts which may aid in correcting this error. For this purpose it is necessary to show that chemical effects (decompositions and combustions) may take place in any part of the spectrum. The points to be established may be thus distinctly stated :—

1st. That so far from chemical influences being restricted to the more refrangible rays, every part of the spectrum, visible and invisible, can give rise to chemical changes, or modify the molecular arrangement of bodies.

2nd. That the ray effective in producing chemical or molecular changes in any special substance is determined by the absorptive property of that substance.

I may here remark that both these propositions were maintained by me many years ago ; an example of the first will be found in the Philosophical Magazine (Dec. 1842), and of the second in a paper in the same journal, "On some Analogies between the Phenomena of the Chemical Rays and those of Radiant Heat" (Sept. 1841).

The opinion commonly held respecting the distribution of chemical force in the spectrum is mainly founded on the behaviour of some of the compounds of silver. These darken when exposed to the more refrangible rays, and, unless correct methods of examination be resorted to, seem to be unaffected by the less refrangible. Hence it has been supposed that in the higher parts of the spectrum a special principle prevails, to which the designation of " actinic rays " is often applied—an inappropriate iteration. In these pages I use the derivatives of ἀκτὶς, not in this restricted sense, but as expressive of radiations of every kind. This is their proper signification.

Every part of the spectrum, no matter what its refrangibility may be, can produce chemical changes ; and therefore there is no special localization of force in any limited region. Out of a

* Communicated by the Author.

large body of evidence that might be adduced, I select a few prominent instances.

1st. *Case of the Compounds of Silver.*

Silver is the basis of the most important photographic sensitive substances. Its iodide, bromide, and chloride, darkening with rapidity under the influence of the more refrangible rays, have mainly been the cause of the misconception above alluded to respecting the tripartite constitution of the spectrum. It is necessary, therefore, to determine what are really the habitudes of these substances.

(1) If a spectrum be received on iodide of silver, formed on the metallic tablet of the Daguerreotype, and carefully screened from all access of extraneous light, both before and during the exposure, on developing with mercury vapour an impression is evolved in all the more refrangible regions. This stain corresponds in character and position to the blackening effect which, under like circumstances, would be found on any common sensitive silver paper. It is this which has given rise to the opinion that the so-called actinic rays exist only in the upper part of the spectrum. If, however, the action of the light be long continued, a white stain makes its appearance over all the less refrangible regions. It has a point of maximum, to which I shall again presently refer.

(2) But if the metallic tablet, during its exposure to the spectrum, be also receiving diffused light of little intensity, as the light of day or of a lamp, it will be found on developing that the impression obtained differs strikingly from the preceding. Every ray that the prism can transmit, from below the extreme red to beyond the extreme violet, has been active The ultra-red heat-lines α, β, γ are present. It must be borne in mind that the impression of these lines is a proof of proper spectrum-action, and distinguishes it from that of diffused light arising either from the atmosphere or from the imperfect transparency of the prism—a valuable indication. The resulting photograph shows two well-marked regions or phases of action. On its general surface, which, having condensed the mercury vapour, has the aspect of the high lights of the Daguerreotype, and forms, as it were, the basis for the spectrum picture, there is in the region of the more refrangible rays a bluish or olive-coloured impression, the counterpart of the result described in the foregoing paragraph. But in the region of the less refrangible rays no mercurial deposit has occurred, the place of those rays being depicted in metallic silver, dark, and answering to the shadows of the Daguerreotype. This protected portion, which stands out in bold relief from the white background,

reaches from a little below G to beyond the extreme red, and encloses the heat-lines above named. They are in the form of white streaks. Though I speak of them as single lines, they are in reality groups, or perhaps bands.

The general appearance of the photograph at once suggests that the less refrangible rays can arrest the action of the daylight, and protect the silver iodide from change. A close examination shows that there are three points, the extreme red, the centre of the yellow, and the extreme violet, which apparently can hold the daylight in check. There are also two intervening ones, in which the actions conspire. The point of maximum protection corresponds to the point of maximum action referred to above in paragraph (1).

(3) If the metallic tablet, previously to its exposure to the spectrum, be submitted for a few moments to a weak light, so that were it developed it would at this stage whiten all over, the action of the spectrum upon it will be the same as in the last case (2). But this change in the mode of the experiment leads to a very important conclusion. The less refrangible rays can reverse or undo the change, in whatever it may consist, that light has *already* impressed on the iodide of silver.

Now, bearing in mind these facts, that the photographic action of diffused light on this iodide is mainly due to the more refrangible rays it contains, we are brought by these experiments to the following conclusions :—

1st. Every ray in the spectrum acts on the silver iodide.

2nd. The more refrangible rays apparently promote the action of the daylight on that substance ; the less refrangible apparently arrest it.

3rd. For the display of this arresting or antagonizing effect, it is not necessary that the less and more refrangible rays should be acting *simultaneously*. An interval may elapse, and they may act *successively*. Hence the effect is not due to the contemporaneous interference of waves of different periods of vibration with one another; the material particles of the changing substance of the silver iodide are involved.

I abstain for the moment from giving further details of these spectrum impressions. That has been very completely done by Herschel, in the case of one I sent him many years ago. His examination of it, illustrated by a lithograph, may be found in the Philosophical Magazine (Feb. 1843). I shall have to return to the subject of the behaviour of silver iodide in presence of radiations on a subsequent page of this memoir.

The main point at present established is this :—that the silver iodide, under proper treatment, is affected by every ray that a flint-glass prism can transmit ; and therefore it is altogether

erroneous to suppose that chemical force is restricted to the more refrangible portions of the spectrum.

2nd. *Case of Bitumens and Resins.*

These substances are of special interest in the history of photography, since, in the hands of Niepce, they were probably the first on which impressions in the camera were obtained and fixed. Their use has been abandoned in consequence, as it seems to me, of an incorrect opinion of their want of sensitiveness. Properly used they are scarcely inferior to chloride of silver.

The theory of their use is very simple. Alcohol, ether, and various volatile oils respectively dissolve certain portions of these substances. If such a solution be spread in a thin film upon glass, as in the collodion operation, and parts of the surface be then exposed to light, the portions so exposed become insoluble in the same menstruum; they may therefore be developed by its use. Practically, care has to be taken to moderate the solvent action, and to check it at the proper time. The former is accomplished by dilution with some other appropriate liquid; the latter, by the affusion of a stream of water.

The substance I have used is West-Indian bitumen dissolved in benzine, and developed by a mixture of benzine and alcohol.

The bitumen solution being poured on a glass plate in a dark room, and drained off as in the operation of collodion, leaves a film sufficiently thin to be iridescent. This is exposed to the spectrum for five minutes, and then developed.

The beginning of the impression is below the line A, its termination beyond H. Every ray in the spectrum acts; the proof is continuous, except where the Fraunhofer lines fall. A better illustration that the chemical action of the spectrum is not restricted to the higher rays, but is possessed by all, could hardly be adduced.

3rd. *Case of Carbonic Acid.*

The decomposition of carbonic acid by plants under the influence of sunshine is undoubtedly the most important of all actino-chemical facts. The existence of the vegetable world, and, indeed, it may be said, the existence of all living things, depends upon it.

I first effected this decomposition in the solar spectrum, as may be found in a memoir in the Philosophical Magazine (Sept. 1843). The results ascertained by me at that time from the direct-spectrum experiment, that the decomposition of carbonic acid is effected by the less, not by the more refrangible rays, have been confirmed by all recent experimenters, who

differ only as regards the exact position of the maximum.   In the discussions that have arisen, this decomposition has often incorrectly been referred to the *green* parts of plants.   Plants which have been caused to germinate and grow to a certain stage in darkness are etiolated; yet these, when brought into the sunlight, decompose carbonic acid, and *then* turn green.   The chlorophyl thus produced is the effect of the decomposition, not its cause.   Facts derived from the visible absorptive action of chlorophyl do not necessarily apply to the decomposition of carbonic acid.   The curve of the production of chlorophyl, the curve of the destruction of chlorophyl, the curve of the visible absorption of chlorophyl, and the curve of the decomposition of carbonic acid are not all necessarily coincident.   To confound them together, as is too frequently done, is to be led to incorrect conclusions.

Two different methods may be resorted to for determining the rays which accomplish the decomposition of carbonic acid.

1st. The place of maximum evolution of oxygen gas in the spectrum may be determined.   2nd. The place in which young etiolated plants turn green.

I resorted to both these methods, and obtained from them the same results.   The rays which decompose carbonic acid are the same which turn etiolated plants green.   They may be designated as the yellow, with the orange on one side and a portion of the green on the other.   Though the form of experimentation does not admit a close reference to the fixed lines, I think we are justified in supposing that the point of maximum action is in the yellow.   It must be borne in mind that the rapidly increasing concentration of the rays occasioned by the peculiarity of prismatic dispersion towards the red end, will give a deceptive preponderance in that direction.   Without entering further into this discussion, it is sufficient for my present purpose to understand that the decomposition in question is accomplished by rays between the fixed lines B and F.

The two absorptive media, potassium bichromate and cupro-ammonium sulphate, so often and so usefully employed in actino-chemical researches, corroborate this conclusion.   Plants cannot decompose carbonic acid, nor can they turn green, in rays that have passed through a solution of the latter salt.   They accomplish both these results in rays that have passed through the former.

The decomposition of carbonic acid, and the production of chlorophyl, by the less refrangible rays of the spectrum, afford thus a striking illustration that chemical changes may be brought about by other than the so-called chemical rays.

4th. *Case of the Colours of Flowers.*

The production and destruction of vegetable colours by the agency of light has, of course, long been a matter of common observation. Little, however, has been done in the special examination of the facts, and that little, for the most part, by Herschel.

We have only to examine his memoir in the Philosophical Transactions (Part II. 1842) to be satisfied that nearly every radiation can produce effects. Thus the yellow stain imparted by the *Corchorus japonica* to paper is whitened by the green, blue, indigo, and violet rays. The rose-red of the *Ten-weeks Stock* is, in like manner, changed by the yellow, orange, and red. The rich blue tint of the *Viola odorata*, turned green by sodium carbonate, is bleached by the same group of rays. The green (chlorophyl) of the *Elder* leaf is changed by the extreme red.

It is needless to extend this list of examples. The foregoing establish the principle that every part of the spectrum displays activity, some vegetable colours being affected by some, others by other rays. It is, however, desirable that the general principle at which Herschel arrived, viz. that the *luminous* rays are chiefly effective, should be more closely examined. Some important physiological explanations turn on that principle. These so-called luminous rays are such as can impress the retina, which, like organic colours, is a carbon compound. There are strong reasons for inferring that carbon is affected mainly by rays the wave-lengths of which are between those of the extreme red and extreme violet, the maximum being in the yellow.

It is, however, to a former experimenter, Grothuss, that we owe the discovery of the law under which these decompositions of the colours of flowers take place. This law in repeated instances was verified by Herschel, and more recently by myself. It may be thus expressed :—"The rays which are effective in the destruction of any given vegetable colour, are those which by their union produce a tint complementary to the colour destroyed." Even the partial establishment of this law, already accomplished, is sufficient to prove that chemical effects are not limited to the more refrangible places of the spectrum, but can be occasioned by any ray.

5th. *Case of the union of Chlorine and Hydrogen.*

In the Philosophical Magazine (December 1843) may be found the description of an actinometer invented by me, depending for its indications on the combination of chlorine and hydrogen, those gases having been evolved in equal volumes from

hydrochloric acid by a small voltaic battery. This instrument, modified to suit their purposes, was used by Professors Bunsen and Roscoe in their photometrical researches. Many of my experiments were repeated by them (Transactions of the Royal Society, 1856, 1857).

In Table III. of my memoir above referred to, it is shown that this mixture is affected by every ray of the spectrum, but by different ones with very different energy. The maximum is in the indigo, the action there being more than 700 times as powerful as in the extreme red.

6th. *Case of the bending of the Stems of Plants in the Spectrum.*

It is a matter of common observation that plants tend to grow towards the light. Dr. Gardner, however, was the first to examine the details of this phenomenon in the spectrum; his memoir is in the Philosophical Magazine (Jan. 1844). When seeds are made to germinate and grow for a few days in darkness, they develope vertical stems some inches in length. These, on being placed so as to receive the spectrum, soon exhibit a bending motion. The stems in other parts of the spectrum turn towards the indigo; those in the indigo bend to the approaching ray. Removed into darkness, they recover their upright position. These movements are the most striking of all actinic phenomena; I have often witnessed them with admiration.

Dr. Gardner's experiments were repeated and confirmed by M. Dutrochet, who, in a report to the French Academy of Sciences (*Comptes Rendus*, No. 26, June 1844), added a number of facts respecting the bending of roots *from* the light, which he found to be occasioned by all the coloured rays of the spectrum.

In Dr. Gardner's paper there are also some interesting facts respecting the bleaching or decolorization of chlorophyl by light. He used an ethereal solution of that substance.

"The first action of light is perceived in the mean red rays; and it attains a maximum incomparably greater at that point than elsewhere. The next part affected is in the indigo; and accompanying it there is an action from $+ 10.5$ to $+ 36.0$ of the same scale (Herschel's), beginning abruptly in Fraunhofer's blue. So striking is this whole result, that some of my earlier spectra contained a perfectly neutral space from -5.0 to $+10.5$, in which the chlorophyl was in no way changed; whilst the solar picture in the red was sharp and of a dazzling white. The maximum in the indigo was also bleached, producing a linear spectrum as follows, ⸻   ⸻, in which the orange, yellow, and green rays are neutral. These, it will be remem-

bered, are active in forming chlorophyl. Upon longer exposure the subordinate action along the yellow &c. occurs, but not until the other portions are perfectly bleached.

" In Sir J. Herschel's experiments there remained a salmon-colour after the discharge of the green. This is not seen when chlorophyl is used, and is due to a colouring-matter in the leaf, soluble in water, but not soluble in ether."

I have quoted these results in detail, because they illustrate in a striking manner the law that *vegetable colours are destroyed by rays complementary to those that have produced them,* and furnish proof that rays of every refrangibility may be chemically active.

At this point I abstain from adding other instances showing that chemical changes are brought about in every part of the spectrum. The list of cases here presented might be indefinitely extended if these did not suffice. But how is it possible to restrict the chemical force of the spectrum to the region of the more refrangible rays, in face of the fact that compounds of silver, such as the iodide, which have been heretofore mainly relied upon to support that view, and in fact originated it, are now proved to be affected by every ray, from the invisible ultra-red to the invisible ultra-violet? how, when it is proved that the decomposition of carbonic acid, by far the most general and most important of the chemical actions of light, is brought about, not by the more refrangible, but by the yellow rays? The delicate colours of flowers, which vary indefinitely in their tints, originate under the influence of rays of many different refrangibilities, and are bleached or destroyed by spectrum-colours complementary to their own, and therefore varying indefinitely in their refrangibility. Towards the indigo ray the stems of plants incline; from the red their roots turn away. There is not a wave of light that does not leave its impress on bitumens and resins—some undulations promoting their oxidation, some their deoxidation. These actions are not limited to decomposition; they extend to combination. Every ray in the spectrum brings on the union of chlorine and hydrogen.

The conclusion to which these facts point is, then, that it is erroneous to restrict the chemical force of the spectrum to the more refrangible, or, indeed, to any special region. There is not a ray, visible or invisible, that cannot produce a special chemical effect. The diagram so generally used to illustate the calorific, luminous, and chemical parts of the spectrum serves only to mislead.

Whilst thus we find that chemical action may take place throughout the entire length of the spectrum, the remarks that have been made in the previous memoir (Phil. Mag. August

1872), respecting the difference of calorific distribution in dispersion and diffraction, apply likewise to the chemical force. To be satisfied of this, it is only necessary to compare photographic impressions given by a prism and by a grating.

I published engravings of such diffraction-photographs in 1844. They are referred to in the Philosophical Magazine (June 1845). As they were obtained on silver plates made sensitive by iodine, bromine, and chlorine, they do not extend to the line F.

I had found that certain practical advantages arise from the use of a reflected instead of a transmitted spectrum. The ruled glass was therefore silvered upon its ruled face with the amalgam, copying the surface perfectly. Of the series of spectra, I used the first.

The fixed lines were beautifully represented in the photographs. They were, however, so numerous and so delicate that I did not attempt to do more than to mark the prominent ones. These were, I believe, the first diffraction-photographs that had ever been obtained. The wave-lengths assigned were according to Fraunhofer's scale, which represent parts of a Paris inch.

The length of the photographic impression given by the prism I was then using, from the line H to the ultra-violet end of the spectrum, was about three times that from H to G; but in the spectrum by the grating, though the exposure was in one instance continued for a whole hour, the impression beyond H was not more than $1\frac{1}{2}$ times that to G. In more moderate exposures, the last fixed line in the photograph was about as far from H on one side as G was on the other. This, therefore, showed very clearly the difference of distribution in the diffraction and dispersion spectra.

On the Chemical Action of Radiations on Substances.

Having offered the foregoing evidence in support of the first proposition considered in this memoir, which was to the effect

"That, so far from chemical influences being restricted to the more refrangible rays, every part of the spectrum, visible and invisible, can give rise to chemical changes, or modify the molecular arrangement of bodies," I now pass to the second, which is

"That the ray effective in producing chemical or molecular changes in any special substance is determined by the absorptive property of that substance."

This involves the conception of selective absorption, as I have formerly shown (Phil. Mag. Sept. 1841). A ray which produces a maximum effect in one substance may have no effect on another. Thus the rays which change chlorophyl are not those which change silver iodide.

In the examination of this subject I shall select two well-known instances, presenting the fewest elements and the simplest conditions. They are (1) the decomposition of silver iodide, the basis of so many photographic preparations; (2) the production of hydrochloric acid by the union of its two contituents, chlorine and hydrogen, a mixture of these gases being exceedingly sensitive to light.

1st. *Of the decomposition of Silver Iodide.*

There are two forms in which the silver iodide has been used for photographic purposes:—(1) when prepared by the action of the vapour of iodine on metallic silver, as in the Daguerreotype tablet; (2) when nitrate of silver is decomposed by iodide of potassium or other metallic iodide. These preparations differ strikingly in their actinic behaviour, the former furnishing by far the most interesting series of facts.

When a polished surface of silver is exposed at common temperatures to the vapour of iodine, it speedily tarnishes, a film of silver iodide forming. This passes through several well-marked tints as the exposure continues and the thickness increases. They may be thus enumerated, in the order of their occurrence:—(1) lemon-yellow, (2) golden yellow, (3) red, (4) blue, (5) lavender, (6) metallic, (7) deep yellow, (8) red, (9) green.

All these films are sensitive. Under the influence of radiations they exhibit two phases of modification:—(1) an invisible modification, which, however, can be made apparent or developed, as Daguerre discovered, by exposure to the vapour of mercury—the iodide turning white by the condensation of mercury upon it wherever it has been exposed to the light, but remaining unacted upon in parts that have been in shadow; (2) a visible modification, which arises under a longer exposure, the iodide passing through various shades of olive and blue, and eventually becoming dark grey.

But though all the variously tinted films of silver iodide are impressionable, they differ greatly in relative sensitiveness when compared with each other. This may be very satisfactorily shown by producing on one silver tablet bands of all the above-named colours—an effect readily accomplished by suitably unscreening successive portions of the tablet during the process of iodizing, and then exposing all at the same time to a common radiation. It will be found, on developing with mercury vapour, that the bands of a yellow colour have been the most sensitive, those of a metallic aspect have been scarcely acted on, and those of other tints intermediately. It is to be particularly remarked that the second yellow, numbered 7 in the above series, is equally sensitive as the first yellow, numbered 2.

From this it appears that the sensitiveness of this form of iodide depends not merely on its chemical constitution, but also on its optical properties.

The explanation of this different sensitiveness in different films of iodide becomes obvious when we cause a tablet, prepared as just described, with tinted bands to reflect the radiations falling on it to another tablet iodized to a yellow colour and placed in a camera. After due exposure and development of both with mercury, it will be found that the image of the first tablet, formed on the second, consists of bands of different shades of whiteness. The yellow parts of the first tablet have scarcely affected the second, but its metallic and blue parts have acted very powerfully. On comparing the first plate and its image on the second together, it will be perceived that the parts that have been affected on the one are unaffected on the other.

It may therefore be inferred that the yellow films are sensitive because they absorb the incident radiation, and the metallic and blue are insensitive because they reflect it.

The effect, in whatever it may consist, which occurs during the invisible modification is not durable; it gradually passes away. If tablets that have received impressions be kept for a time before developing, the images upon them gradually disappear.

On these tablets there is no lateral propagation of effect, nothing answering to conduction.

On examining the operation of a radiation continuously applied to one of these sensitive films, it will be discovered that a certain time elapses (that is, a certain amount of the radiation is consumed) before there is any perceptible effect. When that is accomplished, the radiation affects the film to a degree proportional to its quantity, until a second stage is reached. There is then another pause, followed by the second stage, in which visible modification or chemical decomposition sets in. The film begins to darken; it passes through successive tints, brown, red, olive, blue, and eventually becomes dark grey.

I have described in some of the foregoing paragraphs the action of the spectrum on silver iodide, as presented on the tablet of the Daguerreotype, showing the difference in the impressions obtained, 1st, when extraneous light has been excluded; 2nd, when it has been permitted simultaneously or previously to act.

In the latter case, in all that region of the spectrum from the more refrangible extremity to somewhat beneath the line G, the usual darkening effect, manifested by silver compounds, is observed; but below this, and to the extreme less refrangible rays, with certain variations of intensity, the action of the ex-

traneous and simultaneously acting light is checked, and the effect of previously acting light is destroyed.

It happened that in 1842 I obtained two very fine specimens of the latter spectra : one of these I sent to Sir J. Herschel ; the other is still in my possession.

In the Philosophical Magazine (February 1843) Herschel gave a detailed description of these spectrum-impressions. He was disposed to refer the appearance they present to the phenomena of their films, but at the same time pointed out the difficulties in the way of that explanation.

He also sent me three proofs he had obtained on ordinary sensitive paper, darkened by exposure to light, then washed with a solution of iodide of potassium, and placed in the spectrum. He described them as follows :—

(1) " Blackened paper, from which excess of nitrate of silver has not been abstracted, under the influence of an iodic salt. Produced by a November sun. N.B. View it also transparently against the light."

(2) " Blackened paper, under the influence of an iodic salt, when no excess of nitrate of silver exists in the paper."

(3) " Action of spectrum under iodic influence when very little nitrate of silver remains in excess in the paper. To be viewed also transparently."

These paper photographs I still preserve. They are as perfect as when first made. The different coloured spaces of the spectrum are marked upon them with pencil. The appearances they respectively present are as follows :—

(1) is bleached by the more refrangible rays, and blackened deeply from the yellow to the ultra-red.

(2) is bleached from the ultra-invisible red to the ultra-violet. A maximum occurs abruptly about the blue.

(3) has the same upper spectrum as the others, a bleached dot in the centre of the yellow, and a darkened space in the extreme red. The action has reached from the ultra-red to the ultra-violet.

In Herschel's opinion, these effects in the less refrangible region are connected with the drying of the paper. It is well known that paper in a damp condition is more sensitive than such as is dry.

But obviously this condition does not obtain in the case of the Daguerreotype operation, which is essentially a dry process.

In 1846 MM. Foucault and Fizeau, having repeated the experiment originally made by me, presented a communication to the French Academy of Sciences, to the effect that when a silver tablet which has been sensitized by exposure to iodine and bromine, and then impressed by light, is exposed to the spectrum,

the effect is greatly increased in all the region above the line C, and is neutralized in all that below C. They remarked the distinctness with which the atmospheric line A comes out, and saw the ultra-spectrum heat-rays α, β, γ, described by me some years previously.

The interpretation given by them is, that the more refrangible rays promote the previous action of light, the less neutralize it. The curve representing the chemical intensities of the different rays would cross the axis of abscissæ about the boundary of the red and orange; below that point, to the ultra-red, the ordinates would have negative values; above it, to the ultra-violet, those values would be positive (*Comptes Rendus*, No. 14, vol. xxiii.)

Hereupon M. Becquerel communicated to the same Academy a criticism on this interpretation, the opinion maintained by him being that, while the more refrangible rays excite sensitive surfaces, the less refrangible, far from neutralizing, continue the action so begun. To the former he gave the designation "rayons excitateurs;" to the latter, "rayons continuateurs" (*Comptes Rendus*, No. 17, vol. xxiii.).

In 1847 Mr. Claudet communicated a paper to the Royal Society, subsequently published in the Philosophical Magazine (February 1848), on this subject. His attention had been drawn to it by observing that the red image of the sun, during a dense fog, had destroyed the effect previously produced on a sensitive silver surface, and that this destruction could be occasioned at pleasure by the use of red and yellow screens. A surface which had been impressed by daylight, and the impression then obliterated by less refrangible rays, had recovered its primitive condition. It was ready to be impressed again by daylight; and again the resulting effect might be destroyed. Claudet found that this excitation and neutralization might be repeated many times, the chemical constitution of the film remaining unchanged to the last.

These facts seem to be inconsistent with Herschel's opinion, that positive and negative pictures may succeed each other by the continued action of a radiation, on the principle of Newton's rings.

On a collodion surface such negative neutralizing or reversing actions cannot be obtained by the less refrangible rays. The spectrum-impression, developed in the usual manner by an iron salt, presents a sudden maximum about the line G, and continues thence to the highest limit of the spectrum. In the other direction it extends below F. From E to the ultra-red not a trace of action can be detected. The lines α, β, γ cannot be obtained on collodion. There is therefore a difference be-

tween its behaviour under exposure to light, and that of a Daguerreotype tablet.

The reversals that are obtained on collodion by the use of haloid compounds are altogether different from the reversals on the thin films of a silver tablet. They are produced by the *more* refrangible rays.

On exposing a collodion surface prepared in the usual manner to daylight long enough to stain it completely, then washing off the free nitrate, and in succession dipping the plate into a weak solution of iodide of potassium, exposing it to the spectrum, washing, again dipping it into the nitrate bath, and finally developing, a reverse action is obtained. The daylight is perfectly neutralized, but not after the manner of a Daguerreotype. In the region about G, the place of maximum action in collodion, the impression of the light is totally removed by an exposure of five seconds. In twelve seconds the protected space is much larger; in thirty seconds it has spread from F to H. It is, however, to be particularly remarked that the less refrangible rays show no action.

The results are substantially the same when, instead of iodide of potassium, chloride of sodium, corrosive sublimate, bromide of potassium, or fluoride of potassium is used. In all these the reversing action is from F to H, and has its maximum somewhere about G; that is, the reversing action coincides with the direct action; there is no protection in the lower portion of the spectrum, as in the Daguerreotype. The effect is altogether due to the change of composition of the sensitive film. Ordinarily it contains free nitrate; now it contains free iodide, chloride, &c.

The silver compounds in collodion absorb the radiations falling on them which are capable of producing a photographic effect. Yet, sensitive as it is, collodion is very far from having its maximum sensitiveness, as is shown by the following experiment, which is of no small interest to photographers. I took five dry collodion plates, prepared by what is known as the tannin process, and, having made a pile of them, caused the rays of a gas-flame to pass through them all at the same time. On developing, it was found that the first plate was strongly impressed, and the second (which had been behind it) apparently quite as much; even the fifth was considerably stained. From this it follows that the collodion film, as ordinarily used, absorbs only a fractional portion of the rays that can affect it. Could it be made to absorb the whole, its sensitiveness would be correspondingly increased.

A ray that has suffered complete absorption can bring about no further change; partial absorption, arising from inadequate

thickness, may leave the ray possessed of a portion of its power. There must be a correspondence between the intensity of the incident ray and the thickness of the absorbing medium to produce a maximum effect.

Though the silver iodide is affected by radiations of every refrangibility, it is decomposed (so that a subiodide results) only by those of which the wave-length is less than 5000; if in presence of metallic silver (as on the Daguerreotype tablet), the iodine disengaged unites with the free silver beneath. The rays of high refrangibility occasion in it chemical decomposition, those of less refrangibility physical modification. In the language of the older theories of actino-chemistry, this substance may be said to exert a selective absorption. In this it illustrates the general principle, that it depends on the nature of the ponderable material presented to radiations, which of them shall be absorbed.

2nd. *Of the union of Chlorine and Hydrogen.*

An interesting experiment, illustrating the fact that chlorine gas absorbs the radiations which bring about its combination with hydrogen, may be made by covering a test-tube containing an explosive mixture of equal volumes of those gases with a large jar filled with chlorine. This arrangement may be exposed in the open daylight without risk of exploding the mixture; but if the experiment be made with a covering jar containing atmospheric air instead of chlorine, the gases immediately unite, and commonly with an explosion.

I placed a mixture of equal volumes of chlorine and hydrogen in a vessel made of plate glass, the edges of the pieces being cemented together. This vessel was so arranged on a small porcelain trough containing a saturated solution of common salt that it could be used as a gas-jar. The radiations of a lamp were caused to pass through it so as to be submitted to the selective absorption of the mixture. They were then received on a chlorhydrogen actinometer.

Successive experiments were then made (1) with the radiations of a lamp after passing through the absorption-vessel, (2) with the same radiations after the vessel had been removed.

Two facts were now apparent : 1st, the mixture of chlorine and hydrogen in the absorption-vessel began to unite under the influence of the rays of the lamp; 2nd, the rays which had passed through that mixture had lost very much of their chemical force. It was not totally extinct; but the actinometer showed that it had undergone a very great diminution.

From this it follows that, on its passage through a mixture of chlorine and hydrogen, the radiation had suffered absorption,

and, as respects the mixture under trial, had become deactinized; simultaneously the mixture itself had been affected, its constituent gases uniting; and thus it appears that the radiation had undergone a change in producing a change in the ponderable matter.

The following modification of this experiment shows the part played by the chlorine and hydrogen respectively when they are in the act of uniting :—

(*a*) The glass absorption-vessel above described was filled with atmospheric air, and the chemical force of the radiation passing from the lamp through it was determined. It was measured by the time required to cause the index of the actinometer to descend through one division : this was 12 seconds.

(*b*) The absorption-vessel was now half filled with chlorine, obtained from hydrochloric acid and peroxide of manganese. The chemical force of the ray, after passing through it, was determined as before. It was now represented by $25\frac{1}{2}$ seconds.

(*c*) To the chlorine an equal quantity of hydrogen was added, the absorption-vessel being consequently full of the mixture. The radiation was now passing through a stratum of chlorine diluted with hydrogen; and the point to be determined was whether it had undergone the same or a greater or less loss than in the preceding case, since the chlorine was now uniting with the hydrogen. On measuring the force it was found to be represented by 19 seconds.

(*d*) Lastly, the first (*a*) of these measures was repeated, with the view of ascertaining whether the intensity of the lamp had changed. It gave 12 seconds, as before.

From these observations it may be concluded that the addition of hydrogen to chlorine does not increase its absorptive power. Moreover it is obvious that the action of the radiation is expended primarily on the chlorine, giving it a disposition to unite with the hydrogen, and that the functions discharged by the chlorine and by the hydrogen respectively are altogether different. The ray itself also undergoes a change; it suffers absorption, and loss of a part of its *vis viva*.

As to the ray which is thus absorbed. In 1835 I found that a radiation which has passed through a solution of potassium bichromate could not accomplish the union of chlorine and hydrogen, but one which has passed through ammonio-sulphate of copper could do it energetically. This indicates that the effective rays are among the more refrangible.

On exposing these gases in the spectrum, the maximum action takes place in the indigo rays (Phil. Mag. December 1843).

Recently (1871) some suggestions have been made by M. Budde respecting the action of light upon chlorine.

Admitting the correctness of the theorem that the molecules of most elementary gases consist of two atoms, he conceives that the effect of light on chlorine is to tend to divide or actually to divide its molecules into isolated atoms. These atoms, if the gas be kept in the dark, may reunite into molecules.

The chlorine molecule cannot unite with hydrogen; the chlorine atom can; hence insolation brings on combination.

But if the chlorine be unmixed, there will, as a consequence of insolation, be a certain proportion of uncombined atoms; and from this, together with Avogadro's theorem, is drawn the conclusion that this gas, through insolation, increases in specific volume.

Moreover, as the reunion of the chlorine atoms probably produces heat, rays of high refrangibility will cause chlorine to expand; but it will contract to its original volume when no longer under the influence of light.

In corroboration of this conclusion, Budde found that a differential thermometer filled with chlorine showed a certain expansion when placed in the red or yellow rays; but it gave an expansion six or seven times as great when in the violet rays. With carbonic acid and ether no such effect took place.

It should not be forgotten, however, in considering the bearing of these experiments, that chlorine, merely because it is yellowish green, will absorb rays of a complementary (that is, of an indigo and violet) colour, and become heated thereby.

It has next to be determined whether the points of maximum action (that is, the points of maximum absorption) correspond to the rays of emission of either or both these gases, as they apparently ought to do under Ångström's law: "A gas, when luminous, remits rays of light of the same refrangibility as those which it has the power to absorb."

Of the four rays characteristic of hydrogen, there is one the wave-length of which is 4340. It is in the indigo space.

Plücker gives for chlorine a ray nearly answering to this. Its wave-length is 4338; and also another, 4346, the latter being one of the best-marked of the chlorine lines.

There are therefore rays in the indigo which are absorbed both by hydrogen and by chlorine. The place of these rays in the spectrum corresponds to that in which the gases unite, the place of maximum action for their mixture.

But the absorptive action of chlorine is not limited to a few isolated lines. That gas removes a very large portion of the spectrum. Subsequent experiments must determine whether each of these lines of absorption is also a line of maximum chemical action.

The chlorhydrogen actinometer, referred to in previous para-

graphs as depending for its indications on the union of chlorine and hydrogen, furnishes the means of ascertaining many facts respecting the combination of those substances, since it gives accurate quantitative measures.

By referring to my papers in the Philosophical Magazine (Dec. 1843, July 1844, Nov. 1845, Nov. 1857), it will be found that chlorine and hydrogen do not unite in the dark at any ordinary temperature or in any length of time; but if exposed to a feeble radiation, such as that of a lamp, they are strongly affected. The phenomena present two phases:—1st. For a brief period there is no recognizable chemical effect, a preliminary actinization or (as Professors Bunsen and Roscoe subsequently termed it) photochemical induction taking place; it is manifested by an expansion and contraction of the mixture. 2nd. The combination of the gases begins, it steadily increases, and soon acquires uniformity. In obtaining measures by the use of these gases, we must therefore wait until this preliminary actinization is complete. That accomplished, the hydrochloric acid arising from the union of the gases is absorbed so quickly that the movements of the index liquid over the graduated scale give trustworthy indications.

As regards the duration of the effect produced on the gases by this preliminary actinization, I found that it continued some time—several hours (Phil. Mag. July 1844). Professors Bunsen and Roscoe, however, in their memoir read before the Royal Society, state that it is quite transient (Trans. Roy. Soc. 1856).

This preliminary actinization completed, the quantity of hydrochloric acid produced measures the quantity of the acting radiation. This I proved by using a gas-flame of standard height, and a measuring-lens consisting of a double convex, 5 inches in diameter, sectors of which could be uncovered by the rotation of pasteboard screens upon its centre, the quantity of hydrochloric acid produced in a given time being proportional to the area of the sector uncovered. The same was also proved by using a standard flame, and exposing the gases during different periods of time. The quantity of hydrochloric acid produced is proportional to the time.

The following experiment illustrates the phenomena arising during the actinization of a mixture of chlorine and hydrogen, and substantiates several of the foregoing statements.

The diverging rays of a lamp were made parallel by a suitable combination of convex lenses. In the resulting beam a chlor-hydrogen actinometer was placed, there being in front of it a metallic screen so arranged that it could be easily removed or replaced, and thus permit the rays of the lamp to fall on the actinometer or intercept them.

On removing the screen and allowing the rays to fall on the sensitive mixture in the actinometer, an expansion amounting to half a degree was observed. In sixty seconds this expansion ceased.

The volume of the mixture now remained stationary, no apparent change going on in it. At length, after the close of 270 seconds, it was beginning to contract, and hydrochloric acid to form.

At the end of 45 seconds more a contraction of half a degree had occurred; the volume of the mixture was therefore now the same as when the experiment began, this half degree of contraction compensating for the half degree of expansion.

The rate of contraction of the gaseous mixture (that is, the rate at which its constituents were uniting) was then ascertained.

From these observations it appeared that when chlorine and hydrogen unite under the influence of a radiation, there are four distinct periods of action :—

1st. For a brief period the mixture expands.

2nd. For a much longer period it then remains stationary in volume, though still absorbing rays.

3rd. Contraction, arising from the production of hydrochloric acid, begins; at first it goes on slowly, then more and more rapidly.

4th. After that contraction is fully established, it proceeds with uniformity, equal quantities of hydrochloric acid being produced in equal times by the action of equal quantities of the rays.

The prominent phenomena exhibited by a mixture of chlorine and hydrogen are a preliminary absorption and a subsequent definite action.

It may be remarked, since a similar preliminary absorption occurs in the case of other sensitive substances, that there is in practical photography an advantage, both as respects time and correctness in light and shadow, gained by submitting a sensitive surface to a brief exposure in a dim light, so as to pass it through its preliminary stage.

The expansion referred to as taking place during the first of these periods may be advantageously observed when the disturbing radiation is very intense. It is well seen when a Leyden jar is discharged in the vicinity of the actinometer. Though this light lasts but a very small fraction of a second, it produces an instantaneous expansion, followed by an instantaneous contraction. Not unfrequently the gases unite with an explosion; I have had several of these instruments destroyed in that manner.

It might be supposed that this instantaneous expansion is due to a heat-disturbance, arising from the absorption of rays

that are not engaged in producing the chemical effect. But this interpretation seems to be incompatible with the instantaneously following contraction. Though it is admissible that heat should be instantaneously disengaged by the preliminary actinization, it is difficult to conceive how it can so instantaneously disappear.

When the radiation is withdrawn and the hydrochloric acid absorbed, there is no after-combining. The action is perfectly definite. For a given amount of chemical action an equivalent quantity of the radiation is absorbed.

The instances I have cited in this discussion of the mode of action of radiations are one of decomposition in the case of silver iodide, and one of combination in the case of hydrochloric acid. I might have introduced another, the dissociation of ferric oxalate, which I have closely studied; but it would have made the memoir of undue length. From the facts herein considered, the following deductions may be drawn :—

When a radiation impinges on a material substance, it imparts to that substance more or less of its *vis viva*, and therefore undergoes a change itself.

The substance also is disturbed. Its physical and chemical properties determine the resulting phenomena.

1st. If the substance is black and undecomposable, the radiation establishes vibrations among the molecules it encounters. We interpret these vibrations as radiant heat. The molecules of the medium do not lose the *vis viva* they have acquired at once, since they are of greater density than the ether. Each becomes a centre of agitation; and heat-radiation and conduction in all directions are the result. The undulations thus set up are commonly of longer waves; and as the movements gradually decline the shorter waves are the first to be extinguished, the longer ones the last. This, therefore, is in accordance with what I found to be the case in the gradual warming of a solid body, in which the long waves pertain to a low temperature, the short ones arising as the temperature ascends (Phil. Mag. May 1847).

In some cases, however, instead of the disturbing undulation giving rise to longer waves, it produces shorter ones, as is shown when a platinum wire is put into a hydrogen flame, or by Tyndall's experiment, in which invisible undulations below the red give rise to the ignition of platinum.

2nd. If the substance is coloured and undecomposable, it will extinguish rays complementary to its own tint. Its temperature will rise correspondingly.

3rd. If the substance is decomposable, those portions of the radiation presented to it which are of a complementary tint will be extinguished. The force thus disappearing will not be expended in establishing vibrations in the arresting particles, but in breaking down the union of those which have arrested them from associated particles. No vibrations, therefore, are originated, no heat is produced, there is no lateral conduction.

In actinic decompositions the effects may be conveniently divided into two phases:—1st, physical; 2nd, chemical.

The physical phase precedes the chemical. It consists in a preliminary disturbance of the group of molecules about to be decomposed. Up to a certain point the dislocation taking place may be retraced or reduced, and things brought back to their original condition. But that point once gained, decomposition ensues, and the result is permanent.

I may perhaps illustrate this by a familiar example. If a sheet of paper be held before a fire, its surface will gradually warm; and if the exposure be not too long or the fire too hot, on removing it the paper will gradually cool, recovering its former condition without any permanent change. One could conceive that the laws of absorption and radiation might not only be studied but again and again illustrated by the exposure and removal of such a sheet. But a certain point of temperature or exposure gained, the paper scorches—that is, undergoes chemical change; and then there is no restoration, no recovery of its original condition.

Hence it may be said of such a sheet of paper that it exhibits two phases, in the first of which a return to the original condition is possible; in the second such a return is impossible, because of the supervening of a chemical change.

An investigation of the effects produced by a ray presents, then, these two separate and distinct phases—the physical and the chemical.

General Conclusions.

The facts presented in the former and the present memoir suggest the following conclusions:—

1st. That the concentration of heat heretofore observed in the less-refrangible portion of the prismatic spectrum, arises from the special action of the prism, and would not be perceived in a diffraction-spectrum.

2nd. From the long-observed and unquestionable fact that there is in the prismatic spectrum a gradual diminution in the heat-measures, from a maximum below the red to a minimum

in the violet, coupled with the fact now presented by me, that the heat of the upper half of the spectrum is equal to that of the lower half, it follows that the true distribution of heat throughout the spaces of the spectrum is equal. In consequence of the equal velocity of ether-waves, they will, on complete extinction by a receiving surface, generate equal quantities of heat, no matter what their length may be, provided that their extinction take place without producing any chemical effect.

3rd. That it is incorrect to restrict to the upper portion of the spectrum the property of producing chemical changes. Such changes may be produced by waves of any refrangibility.

4th. That every chemical effect observed in the spectrum is in consequence of the absorption of specific radiations, the absorbed or acting radiation being determined by the properties of the substance undergoing change.

5th. That the figure so generally employed in works on actino-chemistry to indicate the distribution of heat, light, and actinism in the spectrum, serves only to mislead. Its heat-curve is determined by the action of the prism, not by the properties of calorific radiations; its actinic curve does not represent any special peculiarities of the spectrum, but the habitudes of certain compounds of silver.

LIII. *On the Nutrition of Muscular and Pulmonary Tissues in Health and when affected with disease from Phthisis. By* WILLIAM MARCET, *M.D., F.R.S.*

[Concluded from p. 365.]

PART II.

On the Constitution and Nutrition of Pulmonary Tissue in Health.

I HAD been prepared to find that the mode of nutrition of muscular tissue equally applied to pulmonary tissue; but the result of the inquiry showed that there is a difference between the two processes. The investigation was carried on in the same way as in the case of muscular tissue.

Three samples of pulmonary tissue from three different oxen, submitted to analysis, gave the following results :—

Class I.
Composition of Pulmonary Tissue proper or insoluble in water (mature tissue).

	Analysis I.		Analysis II.		Analysis III.		Mean.	
	On 200 grms. tissue.	Propor-tion.	On 200 grms. tissue.	Propor-tion.	On 200 grms. tissue.	Propor-tion.	On 200 grms. tissue.	Propor-tion.
Albuminous material	21·442	100	18·204	100	20·04	100	19·895	100
Phosphoric acid	0·543	2·53	0·432	2·37	0·467	2·33	0·481	2·41
Potash	0·061	0·28	0·043	0·23	0·050	0·25	0·051	0·25

Class II.
Composition of Nutritive Material, entirely colloid.

Albumen	12·93*	100	13·9	100	12·257	100	13·029	100
Phosphoric acid......	0·327	2·53	0·33	2·38	0·285	2·32	0·314	2·41
Potash	0·037	0·28	0·033	0·24	0·030	0·24	0·033	0·25

Class III.
Composition of Effete Material, entirely crystalloid.

Albuminoid material	1·27	100	1·27	100	1·678	100	1·406	100
Phosphoric acid......	0·060	4·72	0·050	3·93	0·063	3·69	0·058	4·11
Potash	0·475	37·40	0·431	33·94	0·447	26·64	0·451	32·66

Proportion of Phosphoric acid and Potash in Effete Material.

	Analysis I.		Analysis II.		Analysis III.		Mean.
	Found.	In 100.	Found.	In 100.	Found	In 100.	
Phosphoric acid	0·060	11·21	0·050	10·40	0·063	12·35	11·32
Potash	0·475	88·79	0 431	89·60	0·447	87·65	88·68

Amount of Water, Fat, Soda, and Chlorine in 200 grms. of Pulmonary Tissue.

	Analysis I. Sheep.	Analysis II. Sheep.	Analysis III. Sheep.	Analysis IV. Lamb.	Mean.
Water......	157·24	157·56	157·84	160·14	158·2
Fat	undetermined	3·56	4·10	5·18	4·28

In 200 grms. Ox-lungs.

	Analysis I.	Analysis II.	Analysis III.	Mean.
Soda	0·453	0·657	0·450	0·520
Chlorine............	0·463	undetermined	0·415	0·439

* There may have been a mere trace of blood in the pulmonary tissue analyzed, but so little that it cannot have interfered practically with the estimation of the albumen, or with the results in other respects.

By referring to the analyses of muscular tissue, it will be seen that there is a marked difference between the composition of flesh and that of pulmonary tissue, the mature tissue of the lungs containing less albumen and much more phosphoric acid than the mature muscular tissue. The effete material of the lungs is very different from that of muscles, the proportion of albuminoid material and potash it contains being much smaller, and that of the phosphoric acid is about ten times less.

In order to establish clearly the difference existing between the composition of muscle and lung, I have constructed the following Table, which shows the mean results of the analyses, in such a way that a mere glance is required to form an idea of the relative composition of these two different tissues.

Mean composition of Muscle and Lung, in 200 grms.

	Mature tissue.		Nutritive material.		Effete material.	
	Muscle.	Lung.	Muscle.	Lung.	Muscle.	Lung.
Albuminous mat[1].	28·070	19·895	5·745	13·029	3·70	1·406
Phosphoric acid...	0·251	0·481	0·051	0·314	0·563	0·058
Potash	0·086	0·051	0·017	0·033	0·764	0·451

In Effete Material.

	Theory.		Found in 100.	
			Muscle.	Lung.
Phosphoric acid	43	Pyrophosphate of potash	42·4	11·32
Potash	57		57·6	88·68

The most interesting fact brought out in this inquiry refers to the proportion found to exist between the phosphoric acid and potash effete in muscles and pulmonary tissue—as, while in the former their proportion is precisely that of pyrophosphate of potash, no such result is obtained in the case of the lungs, where the proportions of these substances exhibit no chemical relation.

This circumstance would appear at first sight to clash with my theory that phosphoric acid and potash must be transformed into crystalloid chemical compounds, with a view to their elimination by a physical process of diffusion ; but a close consideration of the circumstances bearing on the case will show that the present discrepancy is very satisfactorily accounted for. I explained, in a communication to the 'Lancet' for February 2, 1867, how the evolution of carbonic acid from the lungs during respiration was due to the diffusion of the gas from the blood through the moist substance of the pulmonary vesicles, the same theory being given

subsequently by Bert (*Leçons sur la Physiologie comparée de la Respiration,* par Paul Bert, 1870). By passing through the substance of the lung-tissue, carbonic acid must combine with whatever free potash and soda it may contain, and consequently transform most of the potash into crystalloid carbonate of potash ; the potash is therefore removed as phosphate and carbonate, but mostly as carbonate ; while in the case of muscular tissue, the potash is entirely eliminated as phosphate.

Now, does the colloid condition of phosphoric acid and potash found in animal tissues exist in soil, or only in plants ? This is a very interesting question, open to investigation. One thing is certain—that the liquid excreta of animals and other liquid manures are, as a rule, crystalloids. Plants take up the material they require in quantities which have no relation with equivalent proportions, thereby forming colloids; thus, if soil should contain phosphate of soda and soluble potash salts, plants will take up phosphoric acid and potash in quantities utterly at variance with their equivalent weights, leaving behind nearly the whole of the soda as carbonate and chloride. But I have also reason to believe that crystalloid mixtures are transformed, to some extent, into colloids in the earth.

On the Colloid Condition of Plants.

As a rule, the mineral constituents of plants are very much the same as those of animal tissues ; they mostly consist of phosphoric acid, potash, and magnesia, and are very poor in chloride of sodium. Now phosphoric acid and potash are found in a great measure in the colloid state in vegetable as well as in animal tissues. The vegetables I have examined are wheat or wheaten flour, potato, and rice, selecting those mostly used as food for man. It is remarkable that, although the total amount of phosphoric acid and potash they contain varies, still we find, after dialyzing for twenty-four hours a mixture of these materials with water, the same or nearly the same relation to exist between the colloid and total phosphoric acid and the colloid and total potash respectively in each of them. The analysis was conducted in the following way. 100 grammes, say, of wheaten flour, were mixed with enough distilled water for the whole to be nearly liquid ; and this was placed in a dialyzer which was floated for twenty-four hours over a bulk of water equal to eight or ten times that of the contents of the dialyzer. After that lapse of time the volumes of the contents of the dialyzer and of the solution outside were determined. The material in the dialyzer was then dried and incinerated, and the ash was analyzed for the determination of the phosphoric acid and potash. On the other hand, a certain quantity of the

flour was carefully incinerated, and the phosphoric acid and potash were determined in the ash. A correction had to be introduced in the analysis by diffusion, owing to the colloid mass still holding a proportion of diffusible phosphoric acid and potash, depending on the relation existing between the volumes of fluid in and out of the dialyzer. Thus, if the volume of the outside solution was eight times that of the contents of the dialyzer, one eighth of the phosphoric acid found outside the dialyzer would have to be subtracted from the phosphoric acid found in the dialyzer in order to obtain the correct proportion of colloid phosphoric acid. The following Table shows the result of these analyses :—

Phosphoric Acid and Potash, total and colloid, in Flour, Potato, and Rice, in 100 grms.

Total phosphoric acid.	Colloid phosphoric acid.	Total potash.	Colloid potash.
Flour .... 0·3142	0·2062 found	0·1797	0·0557 found
Potato No. I. ... 0·0911	0·0581 ,,	0·5801	0·2175 ,,
Potato No. II. ... 0·111	0·0698 ,,	0·678	0·263 ,,
Rice .... 0·2020	0·1144 ,,	0·0856	0·2828 ,,

Proportions.

	Total. Colloid.	Total. Colloid.
Flour	1 to 0·60 { corrected per volume.	} 1 to 0·21 { corrected per volume.
Potato { No. I.............. / No. II.	1 to 0·58 ,, 1 to 0·54 ,,	1 to 0·28 ,, 1 to 0·24 ,,
Rice	1 to 0·50 ,,	1 to 0·22 ,,
Mean	1 to 0·55 ,,	1 to 0·24 ,,

Found means determined in the colloid fluid, the result is calculated for 100 grms. of substance analyzed, and is not corrected per volume. *Corrected per volume* means after deduction of the amount of diffusible phosphoric acid or potash contained in the colloid fluid.

It must be recollected that in an inquiry of this kind it would be next to impossible to obtain figures agreeing perfectly with each other. Indeed physiological processes do not appear to admit of numerical results being always identically the same in all similar cases.

If we inquire into the total amount of phosphoric acid and potash found for the different articles of vegetable food analyzed, we shall observe it, especially that of the potash, to differ so widely in flour, potato, and rice that no resemblance can be traced between these vegetables on that score. Notwithstanding this fact, the proportion of colloid phosphoric acid and colloid potash to the total phosphoric acid and total potash remains

nearly the same in every one of these analyses, the mean proportion being for 100 grms. :—

Phosphoric acid.		Potash.	
Total.	Colloid.	Total.	Colloid.
1	0·55	1	0·24

This is a very remarkable law of nature, apparently connected with the nutritive properties of the vegetables analyzed. It certainly establishes a very interesting relation between the composition of certain vegetable substances destined to the nutrition of animals.

On the Constitution and Nutrition of Muscular Tissue in Phthisis.

The state of emaciation to which the human body is so frequently reduced in consumption is a certain indication that in that disease the formation of muscular tissue is deficient; and it occurred to me that the nature of the change the nutrition of flesh undergoes in consumption, and the cause of this phenomenon, might be determined by an inquiry similar to that which had been instituted with regard to the nutrition of muscular tissue in health. Of all the symptoms of consumption the wasting of the muscles is one of the most serious; and when attended with a high temperature of the body and a very deficient appetite, which is often the case, there is but little hopes of staying the progress of the disease; while, on the other hand, even where extensive mischief in the lungs is obvious, if the appetite and digestion remain good, with a low temperature, and should there be but little loss of flesh, there is a fair prospect of the disease being arrested.

The subject for our present consideration is the nature of the change in the nutrition of muscular tissue which is productive of the emaciation. Now the nutrition of flesh in consumption may be either abnormal or merely deficient. If it be abnormal, the constituents of muscle will be altered in their relative proportions; should it be merely deficient, the proportions of their constituents will be the same, but their absolute quantities will be less. We find that the nutrition of muscles in consumptive subjects is abnormal as to the quantity and condition of the water present, amounting to 166·5 instead of 154 for 200 grms. of flesh in health. On examining the muscular tissue of consumptive individuals, it will usually be observed, especially when there is much emaciation, to be wet and soft, instead of firm and dry as in the case of death from other diseases. It is therefore fair to conclude that the water in muscular tissue after death from consumption, besides being

in excess, is in a less colloid condition, or, in other words, has not for the other constituents of flesh that peculiar attraction it has in health, and which binds them all together; and it follows, apparently, that the soluble colloid constituents of the muscles of consumptive subjects (those substances which become transformed into flesh) are in a less colloid condition than they would be in healthy flesh. This must check more or less the formation of muscular tissue in phthisis. The water being in excess will, of course, account for a deficiency of the solid constituents. The following is a tabular statement of my analyses of human muscular tissue after death from phthisis. Table A. contains the result of an early series of analyses, and does not include any determinations of potash.

TABLE A.—Analyses of Juice of Flesh after death from Consumption. 200 grms. of muscle extracted by 500 cubic centims. water (calculated for the whole extract, including that retained in the fibres).

	I.	II.	III.	IV.	V.	VI.	VII.	VIII.	Mean.
Albumen	6·130	4·265	3·784	4·534	4·332	3·516	4·458	4·156	4·396
Phosphoric acid		0·441	0·317	0·441	0·365	0·271	0·354	0·475	0·381
Chlorine	0·262	0·341	0·291	0·401	0·466	0·430	0·387	0·390	0·371

TABLE B.—Constitution and Composition of Human Muscular Tissue after death from Consumption*.

Composition of the Tissue insoluble in water and fibrous.			
	Analysis I. On 200 grms. tissue.	Analysis II. On 200 grms. tissue.	Analysis III. On 200 grms. tissue.	Mean.
Albumen, fibrous	20·52	20·52	20·52	20·52
Phosphoric acid	0·160	0·152	0·096	0·103
Potash	0·035	0·043	0·026	0·035

Composition of the Nutritive Material, entirely Colloid.			
Albumen	4·77	4·437	4·437	4·548
Phosphoric acid	0·037	0·033	0·021	0·030
Potash	0·008	0·009	0·006	0·007

Composition of Effete Material, entirely Crystalloid.			
Albuminoid ...	2·7	2·7	2·7	2·7
Phosphoric acid	0·318	0·352	0·327	0·332
Potash	0·418	0·469	0·439	0·442

* The following analysis of human muscular tissue after death from

Table B. (*continued*).

Effete Phosphoric Acid and Potash as Pyrophosphate.								
	Analysis I.		Analysis II.		Analysis III.		Mean.	Theory.
	Found.	In 100.	Found.	In 100.	Found.	In 100.		
Phosphoric acid	0·318	43·2	0·352	42·9	0·327	42·7	42·9	43·0
Potash	0·418	56·8	0·469	57·1	0·439	57·3	57·1	57·0

In this Table the result found for the albumen of the fibrous tissue in analysis No. I. was introduced in Analyses II. and III. The albumen of the nutritive material in analyses II. and III. is the mean of all my determinations of albumen in the extracts.

These analyses show that, although the solid constituents of flesh be deficient, still they exist in the same relative proportion, or nearly so, as in healthy muscle—the phosphoric acid and potash of the effete material bearing to each other precisely the same relation as they do in pyrophosphate of potash, namely 43 to 57, the exact numbers found being 42·9 to 57·1. It follows that in consumption there is no actual change in the relative proportion of the solid substances concerned in the nutrition of flesh. It is remarkable, however, that the flesh of tubercular subjects should be found to contain more water, chlorine, and soda* than muscular tissue does in health.

The proportion of chlorine and soda in healthy muscular tissue and muscle from consumptive subjects is shown in the following Table :—

consumption, which was accidentally omitted in my manuscript, confirms strikingly the results obtained in Table B :—

In 200 grammes.

	Fibr. proper.	Nutritive.	Effete.
Albuminous	20·52	4·437	2·7
Phosphoric acid ..	0·176	0·038	0·346
Potash	0·075	0·016	0·462

		Found.		Theory (Pyrophosph. of potash).
		grm.	per cent.	per cent.
Effete {	Phosphoric acid ..	0·346	42·8	43
	Potash	0·462	57·2	57

This is the eighth analysis of flesh which bears out my theory.
* Chlorine and soda cannot, apparently, be assimilated.

Chlorine and Soda in different samples of Muscular Tissue in Health and after death from Consumption.

In healthy muscular tissue, in 200 grms.			In muscular tissue from consumptive subjects, in 200 grms.		
Analyses.	Chlorine.	Soda.	Analyses.	Chlorine.	Soda.
I. Ox-flesh	0·196	......	I. ...	0·262	
II. „	0·165	......	II. ...	0·341	
III. „	0·184	......	III. ...	0·291	
IV. „	0·132	......	IV. ...	0·401	
V. „	0·173	......	V. ...	0·466	
VI. „	0·212	......	VI. ...	0·430	
VII. „	undeter.	0·279	VII. ...	0·387	
VIII. „	0·210	0·333	VIII. ...	0·390	
IX. „	0·094	0·193	......		0·385
X. „	0·176	0·289	IX. ...	undeter.	0·402
XI. „	0·117	0·172	X. ...	0·355	0·434
XII. Human	0·183	0·155			
Mean	0·167	0·237*	Mean ...	0·369	0·407

This Table shows that muscular tissue in consumption contains rather more than twice as much chlorine and considerably more soda than it does in health; and from what has been stated above, it follows that muscular tissue in phthisis yields more water, and is moreover wetter than healthy flesh, the proportion of water being 154 for 200 of flesh in health, and 166·5 after death from consumption. Now chlorine and soda do not enter into the composition of the completely assimilated muscular tissue; they form part, however, of the constituents of muscles; and it will be interesting to consider how their increased proportion in the muscles of consumptive patients can be accounted for. The various constituents of flesh, in health, including the water, may be considered to be supplied from the blood in the form of molecules, each of them containing certain proportions of these constituents, which may vary in quantity within certain limits. The water, however, is subject to very slight variations, its proportion of 77 per cent. being very tolerably constant. This water binds together in the colloid form the other material which enters into the composition of flesh, so that the constituents of healthy muscle are not wet from the pre-

* These chlorine determinations were made by dialyzing for twenty-four hours a known proportion of the watery extract of a given weight of flesh. A portion of the fluid outside the dialyzer was then evaporated to dryness, and the residue was incinerated, the chlorine being determined volumetrically in the solution of the ash. It was finally calculated for the total volume of the fluid (in and out of the dialyzer). This may be considered as giving very correct results, twenty-four hours being long enough for chlorine to diffuse out of a dialyzer proportionally to volumes. Chlorides may be safely considered as never being colloid.

sence of the water, which acts a part not unlike that it would take in the formation of a jelly.

In consumption it appears that the proportion of water in every flesh-molecule the blood yields is too high, and moreover that the constituents of these molecules are not bound together by water as they should be; at the same time the *chloride of sodium* of the blood begins to diffuse into the tissues by a physical process which had been kept in abeyance during the maintenance of health, passing through the capillary vessels into the flesh, just as it would have done through the diaphragm of a dialyzer into water; hence it is that in consumption, the physical force of matter is gradually overcoming that force which belongs exclusively to life, the nature of which is still a mystery; and the slow ebb of life in phthisis is a gradual return to a purely physical condition.

On Pulmonary Tissue and its Nutrition in Phthisis.

My inquiries into the chemical changes pulmonary tissue undergoes in phthisis has led to some very interesting results.

In health the lungs consist of a tissue, which, from its structure, allows readily of expansion and contraction; this tissue becomes thoroughly permeated with air during respiration, the oxygen of which diffuses through the substance of the lungs and the pulmonary capillary vessels into the blood, the carbonic acid being eliminated from the blood by a similar process. This gaseous diffusion can only take place so long as the soft and delicate walls of the capillary vessels and pulmonary air-vesicles remain physically unaltered; if they should become hardened or changed in any other way, the diffusion of the gases through their substance must be interfered with or entirely checked. Now in the most common form of phthisis, as shown by Dr. Sanderson, the process begins by a new growth of the interstitial tissue of the pulmonary honeycomb, having its seat in the very walls of the blood-vessel. Even at an early stage, the effect of this is not only to diminish the circulation of blood in the affected part, but to render the pulmonary capillaries more and more unfit for the exchange of gases, by diffusion, between the blood and the inspired air. Eventually the air-cells or alveoli in their turn become filled up with a solid material, losing completely their fitness for respiration. Then the tissue softens and breaks down, apparently from the loss of the colloid state of the consolidated pulmonary tissue. It is this last stage which is usually observed after death; and the pulmonary tissue I have submitted to analysis was mostly in that condition. The method of analysis I adopted was precisely the same as that which had been applied to flesh and healthy pulmonary tissue.

Constitution and Composition of Human Pulmonary Tissue (consolidated and softening) in Consumption, in 200 grms.

Composition of the Tissue insoluble in water.					
	Analysis I.	Analysis II.	Analysis III.	Mean.	Mean. On 100 albumen.
Albumen	17·20	14·32	15·63	15·72	100
Phosphoric acid	0·292	0·288	0·286	0·289	1·84
Potash	0·031	0·025	0·026	0·027	0·17

Composition of the material considered nutritive.					
Albumen	7·63	6·98	6·57	7·06	100
Phosphoric acid	0·129	0·140	0·120	0·130	1·84
Potash	0·014	0·012	0·011	0·012	0·17

Composition of the material considered effete, entirely Crystalloid.					
Albuminoid	2·04	2·066	3·059	2·388	100
Phosphoric acid	0·220	0·230	0·377	0·276	11·55
Potash	0·272	0·268	0·365	0·302	12·65

In 200 grammes of Tissue.				
Water	164·00	167·00	164·00	165·00
Fat	4·17	3·31	4·25	3·91
Soda	0·582	0·580	0·471	0·544
Chlorine	0·450	0·470	0·437	0·452

Effete Phosphoric Acid and Potash.							
	Found.	In 100.	Found.	In 100.	Found.	In 100.	Mean. In 100.	Theory. Pyroph. of potash. In 100.
Phosphoric acid	0·220	44·7	0·230	46·8	0·377	50·8	47·7	43
Potash	0·272	55·3	0·268	53·2	0·365	49·2	52·3	57

In analysis No. 1. the soft semifluid portions of tissue were separated as much as possible, and the harder portions only submitted to analysis. In the other two analyses no such selection was made.

The principal and most striking results obtained from the analysis of pulmonary tissue, consolidated and softening, are :—

1st. A considerable reduction in the amount of albumen, phosphoric acid, and potash, both in the insoluble tissue, and in the nutritive material, compared with the amount of these substances in healthy pulmonary tissue, while the proportion of these substances effete and crystalloid in the diseased tissue is considerably increased. This shows a diminished rate of nutrition,

while there is an increase of material to be eliminated, apparently from a deficient action of that process which under normal circumstances causes its removal.

2nd. That the state of semifluidity in which tubercular lungs are usually found after death, is attended with but a trifling increase in the quantity of water beyond the proportion lungs contain in health—water in the normal tissue amounting to 79·1 per cent., and in the diseased to a mean of 82·5 per cent. At first sight this softening appears to be unacountable; but on a closer consideration the fact admits of an explanation. It may be conceived that in the earlier stage of phthisis the adenoid (tubercular) cells are held together by a colloid attraction, but that after a time, and under certain influences which lower the vital power, this colloid attraction becomes lessened and softening takes place.

3rd. The relative proportions of effete phosphoric acid and potash in the three analyses are very remarkable, as they are found to be quite different from what they are in health. In the normal condition, pulmonary tissue contains effete phosphoric acid and potash in the proportion of 11·32 to 88·68, there being a great deal more potash than is necessary for the formation of a pyrophosphate; and I explained how the removal of the potash could be satisfactorily accounted for, by assuming that it was transformed into a carbonate by the carbonic acid formed during the process of respiration. Now as respiration cannot possibly take place in tuberculosis where the pulmonary structure is altered, if my view is correct we shall expect to find in the effete material of tubercular lungs the proportion between the phosphoric acid and potash materially changed; indeed, as these substances must be removed by a process of physical diffusion, we shall conclude that their relative proportions must be such as to form a crystalloid body. The mean relation obtained was,

| Phosphoric Acid . | . | . | 47·7, |
| Potash | . | . | . | 52·3, |

which approximates the formation of a pyrophosphate of potash, requiring

| Phosphoric Acid . | . | . | 43, |
| Potash | . | . | . | 47. |

I have therefore to point out the singular fact that consolidated and softening lungs in phthisis undergo a process of *nutrition* which appears to be closely allied to that of muscular tissue.

Conclusions.

The conclusions I have arrived at from the inquiry which forms the subject of the present paper may be summed up as follows:—

1st. That there is a safe ground for the belief that the elementary physical constitution of muscle, and of other animal tissues, is similar to that of a jelly—with this difference, that it is an organized jelly whose fibrinous or cellular form gives it due tenacity for the performance of its functions; but its water, albumen, and other constituents appear to hold the same physical relation to each other as would water to gelatine in jelly.

2nd. That all tissues are formed of three different classes of substances, namely :—those which constitute the ripe tissue, or the portion of the tissue insoluble in water; next, those constituting the nutritive material of the tissue, which are soluble in water and colloid; and, finally, those of which the effete material is formed; they are soluble in water, crystalloid, and diffusible.

3rd. That the nutritive material and ripe tissue have the same chemical composition, so that the mature tissue is merely an organized form of the nutritive material, the change being purely morphological.

4th. That in muscular tissue the whole of the phosphoric acid is eliminated under the form of a neutral tribasic phosphate of potash. Liebig has shown by chemical tests, in his admirable work on the Chemistry of Food, that this compound really exists in flesh; but the result of my inquiry is that the whole of the phosphoric acid and potash are eliminated in the proper proportions to form exactly either a neutral tribasic phosphate or a pyrophosphate of potash; while at the same time there exist in flesh certain quantities of phosphoric acid and potash which are not in the proportion of a phosphate, and take part in the actual formation of the mature tissue. This is, I believe, the first time it has been shown with mathematical accuracy by a physiological mode of reasoning, if I may so express it, how substances are brought together and combine in obedience to those laws which regulate and maintain the phenomena of life.

5th. That the albuminous constituents of muscular tissue appear to be eliminated, in the process of waste, under the form of kreatine, kreatinine, and other crystalloid substances.

6th. That blood yields to flesh considerably more potash than is required for the formation of muscular tissue, the excess being necessary for the elimination of the phosphoric acid by converting it into a crystalloid phosphate.

7th. That the nutrition of pulmonary tissue differs from that of muscles, from the parenchyma or substance of the lungs containing a much larger proportion of nutritive material and much less waste, showing apparently that the tissue of the lungs undergoes a more rapid nutrition than that of the muscles.

8th. That while in muscles the phosphoric acid and potash are eliminated in the form of a crystalloid phosphate, in pulmo-

nary tissue there is every reason to believe that the potash is eliminated in a very great measure as a crystalloid carbonate, due to the action of the carbonic acid emitted from the blood during its circulation through the lungs. The effete material in muscles contains phosphoric acid and potash in the proportion of 43 to 57, and in lungs in the proportion of 11·32 to 88·68.

9th. That wheaten flour, potato, and rice contain certain proportions of colloid phosphoric acid and colloid potash, which exist in the three kinds of vegetables very nearly in the ratio of one part of total phosphoric acid to 0·55 part of colloid phosphoric acid, and one part of total potash to 0·24 part of colloid potash—thus establishing the remarkable fact that, at all events in the three above kinds of vegetable food, although the proportion of phosphoric acid and potash respectively differ, still the proportion of total to colloid phosphoric acid and potash in each of them remains very nearly the same.

10th. That in phthisis a given weight of muscular tissue contains less nutritive material than it does in health, less mature or insoluble tissue, rather more water, and a much higher proportion of chlorine and soda.

11th. That, in phthisis, the phosphoric acid and potash effete in muscular tissue are present exactly in the right proportion for the formation of a pyrophosphate, as occurred in healthy flesh. This shows that the process of waste of muscles in phthisis takes place precisely as it did while in the state of health, and confirms the result relative to the composition of the effete material of muscular tissue, eight analyses of flesh yielding phosphoric acid and potash effete in the proportion of a pyrophosphate.

12th. That the emaciation in phthisis appears due mainly to the blood not being in the proper condition to supply nutritive material to muscular tissue. The damp or wet state peculiar to muscles after death from phthisis appears to show that the colloid state of flesh in that disease is somewhat deficient.

13th. That the tubercular or adenoid formation in pulmonary tissue actually undergoes nutrition, and is consequently *a growth*, the phosphoric acid and potash being apparently eliminated, as in the case of flesh, under the form of a crystalloid phosphate. The nutrition of the abnormal growth accounts for the absence of any smell of decomposition, which is nearly invariably observed at the post-mortem examination when performed shortly after death from consumption.

14th. The process of softening of the tubercular substance appears due to a loss of colloid power; it can hardly be owing to an increase in the proportion of water, as there is but very little more water in softening tubercular lungs than in healthy lungs—the proportion being, for 200 grammes of tissue, 158 in

healthy lungs to 165 in pulmonary tubercular growth, partly softening, partly consolidated.

15th. That there is apparently no increase of fat in tubercular pulmonary tissue, there being a mean of 4·28 of fat in 200 grammes of healthy lungs, and 3·91 in a similar weight of the diseased tissue; but as there is a little more water in the diseased than healthy lungs, it follows that a given weight of tubercular matter from the lungs apparently contains, proportionally to its dry residue, a little more fat than healthy pulmonary tissue under a similar circumstance.

16th. That in nature soluble matter is undergoing a perpetual transformation—taking place, in rotation, from the crystalloid into the colloid condition, and from the colloid into the crystalloid condition. Animal secretions and the products of decomposition of animal and vegetable tissues are crystalloid, admitting of their ready distribution through land and water by a physical process of diffusion. These crystalloid substances are transformed into colloids by plants and used in that form as food for animals; and both plants and animals yield them back again in their original crystalloid condition. Chloride of sodium alone appears to be an exception to this rule.

LIV. *On the Laws of Cooling.*
By MM. Jamin *and* Richard*.

Part II. *Cooling Power of Gases†.*

AT the meeting of the 15th July last we announced to the Academy that a gas heated to $\theta + \delta\theta$ in an enclosure the walls of which are at $\theta°$ cools regularly, and loses during each unit of time a quantity of heat expressed by the law which Dulong and Petit found for solid bodies, and which is

$$q = SKH^{c'}\delta\theta^{a'},$$

S expressing the surface of the enclosure, and K a coefficient depending on the gas.

We shall now inquire what is the heat which a gaseous mass takes away, by its contact, from a heated solid placed in its centre. The apparatus remains the same. It is a large balloon of glass immersed in a trough filled with water continually agitated by a current of air, and kept at a sensibly constant temperature θ. The initial pressure H is given by a mercury manometer, and the increments of pressure h by a water manometer.

The heated body is no longer a thermometer, but a platinum wire traversed by an electric current; it does not gradually cool, but, on the contrary, remains at a constant temperature and emits an invariable quantity of heat. Of this the gas takes a portion, is heated, and its pressure augmented to a limit h. When the stationary condition is reached, the heat q' taken by the gas from the wire is equal to that which it yields to the sides of the enclosure, or $= q$. As the latter is known, the former can be found, viz. the velocity of cooling of the wire as a function of its excess t of temperature and of the pressure H of the gas.

I. To arrive at this it is necessary first to measure the excess of temperature t of the wire. Now we know that the electrical resistance of platinum increases with the temperature. Numerous experiments, which will shortly be published, on this subject have been performed in my laboratory by M. R. Benoît: they have shown that, by a valuable exception, the resistance of this metal increases proportionally to the temperature as far as the volatilization of sulphur, and probably beyond; so that the resistances r and r' (at θ and at $t+\theta$) are

$$r = r_0(1+\mu\theta), \quad r' = r_0[1+\mu(t+\theta)];$$

consequently

$$r' - r = r_0\mu t.$$

The augmentation of the resistance of the platinum wire is therefore proportional to its excess of temperature t, which will be known in degrees Centigrade when we have determined r_0 and μ. We shall see that this determination is unnecessary, that we need only express t by the values of $r'-r$, which are proportional to it—which merely amounts to changing the thermometric scale.

To measure the increment of resistance $r'-r$, the electric current is divided into two branches, both of which pass first through copper wires of large section, of little resistance, and equal, wound the same number of times round a differential compass, but in opposite directions. The first branch is then continued by the balloon-wire r', and the second by a rheostat with a mercury cursor (constructed after the pattern devised by Pouillet), and by a second wire identical with the wire in the balloon, but immersed in water at θ and retaining its resistance r. When the current passes, the needle is deflected; we bring it back to zero by adding a length of rheostat which compensates and measures the increment of resistance $r'-r$, and consequently the excess of temperature t.

II. While the current is circulating in the wire and giving it an excess of temperature $r'-r$, the pressure of the gas rises to a limit $H+h$. If we change the intensity of the current, $r'-r$

and h vary at the same time. They both vary equally if we change the initial pressure H. We may make abstraction of the current (which is here only a means for producing the heat), and say that $r'-r$ is a function of H and of h which it is the question to determine.

For this purpose several series of observations were made under different initial pressures H (for example 814 millims., 788 millims., ... 73·9 millims.); and in each case the number of elements of the pile was varied from 5 to 20, which gave as many pairs of values of h and $r'-r$ as there were observations. Taking one of these series (for example, that upon hydrogen under the pressure of 788·4 millims.), and constructing the series of points which have for abscissæ log h, and for ordinates log $(r'-r)$, the tracing shows immediately that all these points arrange themselves in a very regular straight line which makes with the axis of the abscissæ an angle whose tangent is 1·02; its equation is

$$\log (r'-r) = \log A + 1.02 \log h. \quad . \quad . \quad . \quad (2)$$

If we now pass from this series to those corresponding to other pressures, from 814·5 millims. to 73·9 millims., we find in each case a right line parallel to the preceding; they all make with the axis of the abscissæ an angle whose tangent is 1·02. These results are recorded in the following Table, which gives a summary of a portion of the experiments we have made upon hydrogen. The accordance between calculation and experiment is rigorously maintained.

Cooling of the wire in hydrogen.
($d=1·17$; $c=0·37$; $n=0·000267$.)

H = 814·5.			H = 788·4.			H = 656·2.			H = 587·7.			H = 543·2.		
h.	$r'-r$.		h.	$r'-r$.		h.	$r'-r$.		h.	$r'-r$.		h.	$r'-r$.	
	Obs.	Calc.		Obs.	Calc.		Obs.	Calc.		Obs.	Calc.		Obs.	Calc.
222	1920	1865	217	1847	1862	196	1921	1921	180	1941	1908	192	2344	2389
161	1352	1060	156	1346	1341	127	1244	1229	122	1271	1271	138	1729	1704
100	817	817	102	881	869	74	683	693	70	685	710	93	1132	1134
33	256	260	32	255	261	29	225	263	27	244	263	49	610	590

H = 481·3.			H = 407·3.			H = 313·2.			H = 182·3.			H = 73·9.		
176	2408	2363	164	2509	2538	140	2617	2617	99	2931	2962	59	3588	3805
133	1794	1774	122	1859	1873	104	1952	1934	72	2108	2145	42	2729	2718
91	1171	1197	80	1225	1220	70	1294	1281	49	1528	1447	29	1852	1852
50	631	640	44	673	661	40	718	718	29	825	832	11	1224	,,

It is now proved that the coefficient of log h is independent of the pressure; but as the various right lines differ by their ordinate at the origin, log A must be a function of H, a function we will now seek.

If, for that purpose, we give to log h any constant value whatever, for example 2·000, and take from the various right lines the corresponding value of log $(r'-r)$ for the pressure H to which each line corresponds, we get

$$\log A = \log (r'-r) - 1·02 \times 2·000.$$

We next construct these values of log A, taking H for abscissa, and obtain as many points as there were values of H. Now these points arrange themselves again in a very well-drawn right line which makes with the axis of the abscissæ an angle whose tangent is 0·88. Its equation is

$$\log A = \log k - 0·88 \log H.$$

The following numbers show the agreement of the observations and the calculation :—

H.				$r'-r.$	
				Observed.	Calculated.
814·5	.	.	.	819	819
788·4	.	.	.	847	847
656·2	.	.	.	954	971
587·7	.	.	.	1024	1084
543·2	.	.	.	1203	1163
481·3	.	.	.	1320	1311
407·3	.	.	.	1526	1498
313·2	.	.	.	1843	1868
182·3	.	.	.	2988	2954
73·9	.	.	.	6494	6637

Supposing now that H and h are both variable, on substituting for A in equation (2) its value we have

or

$$\log (r'-r) = \log k - 0·88 \log H + 1·02 \log h, \quad . \quad (3)$$

$$r'-r = k \frac{h^{1·02}}{H^{0·88}} :$$

k is a constant determined by the whole of the measurements.

On making the same observations for air and carbonic acid, we arrived at the same formula,

$$r'-r = k \frac{h^{\alpha}}{H^{\beta}}.$$

The following are the values of α and β :—

			Carbonic acid.	Air.	Hydrogen.
α	.	.	0·79	0·88	1·02
β	.	.	−0·61	−0·80	−0·88

III. We now, by a perfectly natural deduction, arrive at the law of cooling. Indeed, since the gas when it has attained its stationary temperature $\theta + d\theta$ takes from the wire a quantity of heat q' equal to that which it yields to the outer wall, it is only necessary to make q and q' equal. Now the quantity q is known: according to our previous memoir it is equal to $\beta k' \dfrac{h^{d'}}{\mathrm{H}^{d'-c'}}$; or, taking logarithms,

$$\log q = \log \beta k' + d' \log h - (d' - c') \log \mathrm{H}. \quad . \quad . \quad (4)$$

But we have just found by experiment

$$\log (r' - r) = \log k + \alpha \log h - \beta \log \mathrm{H}. \quad . \quad (3)$$

Eliminating h between these two equations, we shall have q as a function of $r' - r$ and of H—that is, the law of cooling. This elimination conducts to an equation of the form

$$\log q = \log n + d \log (r' - r) + c \log \mathrm{H}, \quad . \quad . \quad . \quad (5)$$

and, passing again to numbers,

$$q = n(r' - r)^d \mathrm{H}^c, \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad . \quad (6)$$

which is precisely the law of Dulong and Petit, found again by an entirely different process. These are the values of the constants :—

				Carbonic acid.	Air.	Hydrogen.
h	.	.	.	0·922	2·57	26·70
c	.	.	.	0·37	0·44	0·42
d	.	.	.	1·17	1·28	1·30

The exponents differ little from those found by Dulong and Petit. We shall not now insist on this point, as we believe that they are variable; we purpose to return to it in an early communication.

LV. *On the Specific Heat of Carbon at High Temperatures. By* James Dewar, *F.R.S.E., Lecturer on Chemistry, Edinburgh.*

To the Editors of the Philosophical Magazine and Journal.

Gentlemen, Edinburgh, October 25, 1872.

THE following paper contains a few details of some of the experiments on which I founded the communication to the British Association at Brighton, on the Specific Heat of Carbon at High Temperatures, that has seemingly caused some unnecessary annoyance to Dr. H. F. Weber, of Berlin.

So far as I am aware, very few observations have been made in this direction since Pouillet's well-known research on the Specific Heat of Platinum. It was with the object of filling up this

gap that I thought of making a series of experiments, employing the boiling-points of sulphur, cadmium, and zinc as fixed points.

The experiments on carbon were made before I became acquainted with Dr. Weber's paper, and were in reality undertaken as part of a communication made to the Royal Society of Edinburgh on the 1st of April, 1872, entitled, "On recent Estimates of Solar Temperature." An abstract of Dr. Weber's excellent paper appeared in the 'Chemical Journal' for the month of July; and I am surprised he should have overlooked this fact before charging the entire Chemical Section of the British Association with a startling amount of ignorance regarding the progress of scientific research in Germany.

<div align="right">Yours respectfully,</div>

<div align="right">JAMES DEWAR.</div>

As carbon, of all known elementary bodies, is the most refractory, it would be a matter of some interest at the present time to arrive at some approximate knowledge regarding its boiling-point. If this could be defined within reasonable limits, it would be a strong argument against those enormously high temperatures recently attributed to the sun by Secchi and Waterston.

In order to acquire some idea of the highest temperatures produced by chemical action, suppose we calculate the hypothetical maximum temperatures that could be produced during the formation of some of the stable oxides—that is, neglecting dissociation and any increase in the specific heat of the product at high temperatures. On this supposition it is easy to show that silicon would give in oxidizing $19,500^\circ$ C., aluminium $15,000^\circ$ C., and magnesium about $14,600^\circ$ C. As these are the highest results that can be obtained, we may conclude that direct chemical energy could not produce a temperature above $15,000^\circ$ C. in the case of the formation of binary compounds; and as more complicated groupings are generally less stable at high temperatures, we may regard this point as a maximum. It is worthy of remark that the above products form a large portion of the earth's crust; and the observation of Despretz, that magnesia, when exposed to the temperature of the electric arc, only became partially fused, and did not, like all the other substances experimented on, volatilize, strongly supports the stability of this oxide at very high temperatures.

Andrews showed many years ago that, in the case of a metal forming two oxides in the same physical condition, the number of heat-units generated in passing from the metal to the first oxide was nearly identical with those obtained in the combustion of the first oxide itself; in other words, the thermal energy is

proportional to the number of oxygen atoms combined. The two metals experimented on by Andrews were copper and tin; but recently Thomson has shown the same law is applicable to the oxides of nitrogen, and even to the oxides of manganese. The numbers obtained, starting from the first oxide in the latter series, diminish by a constant quantity.

The author has communicated some preliminary experiments to the British Association which induce him to believe the same law is applicable in the negative direction, viz. to the oxides of chlorine.

We are therefore justified in concluding that the two oxides of carbon ought either to have the same thermal value, or the first one may evolve 15,000 or 16,000 units more per oxygen atom than the second. Taking into consideration the general chemical analogies existing between the oxides of tin and the oxides of carbon, we may in the first instance suppose the thermal value identical. It is to be observed that the fact of carbonic oxide being an incondensible gas, and carbonic acid a condensible one, does not interfere with the applicability of Andrews's law, as the total latent heat of vaporization of each substance would be nearly identical.

This is confirmed by noting that the following total latent heats of elements and compounds do not differ much, although their physical properties are very different :—

$$
\begin{array}{lr}
\text{CO}^2, \text{ solid} & 6100 \\
\text{SO}^2, \text{ liquid} & 5600 \\
\text{N}^2\text{O, liquid} & 4400 \\
\text{CS}^2 & 6500 \\
\text{Br} & 4000 \\
\text{I} & 5500 \\
\text{H in Pd} & 4200 \\
\end{array}
$$

As a fair mean we may accept 4000 units as the absorption value per atom for real latent heat of vaporization; and as the latent heat of fluidity in the case of non-metallic bodies is very small, we may suppose it included in the above value. Taking now the thermal values of the oxides of carbon, we have

$$
\begin{array}{ll}
\text{C, O}^2 & = 96{,}000 \\
\text{CO, O} & = 68{,}000 \\
\end{array}
$$

therefore $\quad$ C, O $\quad = 28{,}000$

The difference, therefore, between CO, O − C, O = 40,000 units, which must be regarded as the total number of heat-units required to raise 12 grammes of carbon into the gaseous state. If we deduct from this number 4000 units for liquefaction and evapori-

zation, we have 36,000 units left to be expended in raising the carbon to its boiling-point. Despretz has shown that carbon does not liquefy in the ordinary sense, but passes at once into the form of vapour, so that we may consider it solid up to its point of vaporization.

It now only remains to ascertain the rate at which the specific heat of carbon varies with the temperature; and it will be shown in the sequel that the mean specific heat between 0° and 2000° C. is 0·42. The amount of heat required to raise the 12 grammes of carbon 1° C. is therefore $0·42 \times 12 = 5·04$; and the approximate boiling-point is thus $36,000 \div 5·0 = 7200°$ C. If we give the first oxide 16,000 units in excess of the second, the boiling-point would then only reach 10,000° C.; and it would be difficult to make it any higher by more favourable suppositions based on analogy.

The first series of experiments on the mean specific heat of carbon at high temperatures were made between 1040° C. and 20° C. For this purpose a large plumbago crucible, holding about 30 lbs. of zinc, was heated up to the boiling-point by means of a smith's forge and kept continuously boiling by means of a regulated blast. Into this bath wrought-iron tubes about half an inch in diameter were inserted, with ground iron stoppers for the purpose of holding the pieces of carbon. The part of the tubes above the surface of the zinc have each a short screw turned on the outside in order to attach a plate of sheet iron 8 inches in diameter with rapidity before the tube is removed. The object of the plate is to prevent any particle of zinc adhering to the tube being thrown, in the sudden transference along with the carbon, into the calorimeter, and to prevent radiation. The tubes remain in the bath about twenty minutes; the plate of iron is then screwed on; the smith catches the iron tube near the surface of the zinc with a pair of tongs while the iron stopper is removed; and the carbon is instantly transferred to the calorimeter. The pieces of carbon are not broken or disintegrated by the action of the water; nor is there any steam generated. After the experiments they were dried and weighed. The experiments have always been made with the purest French gas-retort carbon, selecting pieces as free as possible from ash. The calorimeter was carefully surrounded with three cylindrical rings, of which the two outer ones were filled with water in order to keep the temperature of the interior constant. The following Table contains the experimental results of a few observations executed in the above manner:—

<div align="center">

TABLE I.

Mean Specific Heat of Carbon to Boiling-point of Zinc (1040° C.).
Calorimeter and water equivalent to 514·5 grm. units.

</div>

	Weight of carbon.	Initial tempera-ture.	Final tempera-ture.	Increase of tempe-rature.	Specific heat.
I.	4·21	17·88	20·50	2·62	0·314
II.	4·04	16·16	18·67	2·51	0·312
III.	4·20	13·62	16·25	2·63	0·314
IV.	5·292	13·92	17·20	3·28	0·311
V.	5·332	14·22	17·56	3·34	0·315
VI.	4·890	13·40	16·46	3·06	0·314
VII.	2·021	17·08	18·37	1·29	0·318
VIII.	4·190	15·80	18·45	2·65	0·318
IX.	5·760	15·75	19·30	3·55	0·310—Graphite.
X.	2·940	16·34	18·42	2·08	0·356—Cocoa-nut charcoal.
XI.	0·8364	14·22	14·83	0·61	0·366—Diamonds (black).

The mean specific heat of gas-carbon between 1040° C. and 20° C. may be taken as 0·32.

In order to try and find the specific heat of carbon at the highest temperature we can in any way define with accuracy, a series of tentative experiments were made with different forms of crucible to ascertain the shape best adapted for observation with the oxyhydrogen blowpipe. After a number of trials, a cubical block of lime, 2 inches in the side, pierced with two channels one fourth of an inch wide at right angles to each other through the middle of the mass, was found the most convenient form of apparatus. The directions of the channels were inclined so as to meet in the centre of the cube; and in general only one of them passed completely through the mass. The carbon was placed at the juncture of the two channels; and two powerful oxyhydrogen blowpipes had the apices of their cones meeting at this point. After the interior was at a white heat, the carbon was inserted and kept as long as possible. The mass of lime was then lifted and the piece of carbon dropped into the calorimeter.

Bunsen's elegant experiments on the temperature of combustion of hydrogen and oxygen under a pressure of ten atmospheres define the limit as 2800° C.; but in the ordinary oxyhydrogen flame the temperature does not reach 2500° C., according to the observations of Deville and Debray.

On several occasions platinum was fused in the lime cube and thrown into the calorimeter. From several concordant observations the temperature in no case was found to exceed 2100° C.*

* By transferring fused platinum the following results were obtained:—6·75 grms. gave in one experiment 697 grm. units, and 6·7 grms. of metal gave in a second observation 672 grm. units. The latent heat of platinum was taken as 12 grm. units, and the mean specific heat as 0·042.

The following Table contains some of the results obtained in working with this form of apparatus :—

TABLE II.

Mean Specific Heat of Carbon up to temperature of Oxyhydrogen Blowpipe (2000°).

Calorimeter and water equivalent to 523·4 gramme units.

	Weight of carbon.	Initial temperature.	Final temperature.	Increase of temperature.	Increase of temperature, per gramme weight.
I.	0·747	17°54	18°59	1°05	1°40
II.	0·792	17·28	18·47	1·19	1·50
III.	0·741	17·38	18·44	1·06	1·43
IV.	0·3915	17·52	18·12	0·60	1·53
V.	0·14	17·92	18·15	0·23	1·64

Calculating from the highest result obtained at a temperature of 2000° C., the mean specific heat of carbon is about 0·42. The true specific heat at 2000° must be at least 0·5 ; so that at this temperature carbon would agree with the law of Dulong and Petit. In general the rate at which the specific heat varies in the case of the metals may be represented by a straight line ; and the increment seems to be directly related to the rate of variation of the coefficient of expansion. Now in the case of diamond, graphite, and gas-carbon $\dfrac{\Delta\phi}{\Delta\Theta}$ are as the numbers 4·32 : 3·03 : 3·3, according to Fizeau ; and as he has further shown that diamond has a minimum volume at −42°·3 C., and that below this temperature it expands as the temperature falls, we may anticipate some marked alteration in the specific heat at very low temperatures, which Dr. Weber proposes to investigate. Of the three varieties of carbon, graphite is certainly the most stable at very high temperatures. Gas-retort carbon, after being used as poles in a powerful electric arc, is in part transformed into graphite ; and the diamond exposed to the temperature of the voltaic arc passes also into graphite. Unless graphite or carbon can pass into the form of diamond under certain conditions of pressure at comparatively low temperatures, or is of vegetable origin, it is difficult to conceive how diamond could occur if this earth ever had a temperature as high as that of the voltaic arc. Starting from absolute zero, carbon as graphite most probably increases regularly in specific heat, whereas diamond probably diminishes until we reach −42°·3 C., and then increases regularly until it exceeds that of graphite, which it continues to do until they agree at very high temperatures. The excess of heat taken in by the diamond accumulates

until it is sufficient to produce a change of state, so that it is not taken in at one particular temperature, as occurs in the case of allotropic phosphorus, but throughout a long range. If we could define the point where the change referred to takes place, we might, by keeping carbon near this temperature, change it partially into diamond. But all these questions must remain without any decisive answer until we know with certainty the heat of combustion of graphite and diamond.

LVI. *Notices respecting New Books.*

The Strains in Trusses computed by means of diagrams : with twenty examples drawn to scale. By FRANCIS A. RANKEN, *M.A., C.E., Lecturer at the Hartley Institution, Southampton, formerly Assistant Engineer on the Cambrian Railways, &c.* London: Longmans, Green, and Co. 1872. (Pp. 64, 8vo.)

THE object of this work is to illustrate, by a variety of examples, the solution of the following problem :—Given that a number of points in one plane are joined two and two by straight lines in such a way as to form a rigid system, and that forces act in the same plane at these points in such a way as to hold the system in equilibrium, to determine the forces transmitted along the lines, and whether they tend to stretch or compress them severally. It is scarcely necessary to add that this is the question presented for solution when trussed roofs are designed and all considerations of transverse strain are put on one side. The book consists mainly of examples ; and the solutions are effected entirely by means of the triangle and polygon of forces, without calculation, merely by means of a construction made with scale and compasses. The general question is one which lends itself very readily to the method of solution adopted ; and though in some cases, where the truss is of a complicated form, the diagram giving the solution is intricate, yet the result is obtained by very simple means, and does away with the need of a most laborious though not otherwise difficult calculation.

The number of cases actually solved is very considerable ; in fact all the ordinary forms of trusses are discussed, and the diagrams which yield the solution drawn carefully to scale.

The book will doubtless prove useful to students of Engineering, and might be studied with advantage by all who are going through a course of Theoretical Mechanics. While fully recognizing the great care that has been bestowed upon the diagrams, we may perhaps add that it would have been of service to beginners if Mr. Ranken had shown (say in one diagram) all the forces which actually keep each joint in equilibrium. To have done this throughout the book would have made the diagrams unnecessarily complicated ; but it might have been done, for instance, in the diagram on p. 18, and would have helped the student to understand the action of the forces in this and other cases. When several forces are shown, a beginner is very apt to think that they are all he is concerned with.

LVII. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

[Continued from p. 394.]

June 20, 1872.—Sir James Paget, Bart., D.C.L., Vice-President, in the Chair.

THE following communications were read :—

"Volcanic Energy : an attempt to develope its true Origin and Cosmical Relations." By Robert Mallet, F.R.S.

The author passes in brief review the principal theories which in modern times have been proposed to account for volcanic activity.

The chemical theory, which owed its partial acceptance chiefly to the fame of Davy, may be dismissed, as all known facts tend to show that the chemical energies of the materials of our globe were almost wholly exhausted prior to the consolidation of its surface.

The mechanical theory, which finds in a nucleus still in a state of liquid fusion a store of heat and of lava &c., is only tenable on the admission of a *very thin* solid crust ; and even through a crust but 30 miles thick it is difficult to see how surface-water is to gain access to the fused nucleus ; yet without water there can be no volcano. More recent investigation on the part of mathematicians has been supposed to prove that the earth's crust is not *thin*. Attaching little value to the calculations as to this based on precession, the author yet concludes, on other grounds, that the solid crust is probably of great thickness, and that, although there is evidence of a nucleus much hotter than the crust, there is no certainty that any part of it remains liquid ; but if so, it is in any case too deep to render it conceivable that surface-water should make its way down to it. The results of geological speculation and of physico-mathematical reasoning thus oppose each other ; so that some source of volcanic heat closer to the surface remains to be sought. The hypothesis to supply this, proposed by Hopkins and adopted by some, viz. of isolated subterranean lakes of liquid matter in fusion at no great depth from the surface remaining fused for ages, surrounded by colder and solid rock, and with (by hypothesis) access of surface-water, the author views as feeble and unsustainable.

A source, then, for volcanic heat remains still to be found ; and if found under conditions admitting to it water, especially of the sea, all known phenomena of volcanic action on our earth's surface are explicable.

The author points out various relations and points of connexion between volcanic phenomena, seismic phenomena, and the lines of mountain elevation, which sufficiently indicate that they are all due to the play of one set of cosmical forces, though different in degree of energy, which has been constantly decaying with time.

He traces the ways in which the contraction of our globe has been met, from the period of its original fluidity to the present state :— first by deformation of the spheroid, forming generally the ocean-basins and the land ; afterwards by the foldings over and elevations of the thickened crust into mountain-ranges &c. ; and lastly by the

mechanism which he points out as giving rise to volcanic action. The theory of mountain-elevation proposed by C. Prévost was the only true one,—that which ascribes this to tangential pressures propagated through a solid crust of sufficient thickness to transmit them, those pressures being produced by the relative rate of contraction of the nucleus and of the crust: the former being at the higher temperature, and having a higher coefficient of contraction for equal loss of heat, tends to shrink away from beneath the crust, leaving the latter partially unsupported. This, which during a much more rapid rate of cooling from higher temperature of the whole globe and from a thinner crust gave rise in former epochs to mountain-elevation, in the present state of things gives rise to volcanic heat. By the application of a theorem of Lagrange, the author proves that the earth's solid crust, however great may be its thickness, and even if of materials far more cohesive and rigid than those of which we must suppose it to consist, must, if even to a very small extent left unsupported by the shrinking away of the nucleus, crush up in places by its own gravity and by the attraction of the nucleus.

This is actually going on; and in this partial crushing, at places or depths dependent on the material and on conditions pointed out, the author discovers the true cause of volcanic heat. As the solid crust sinks together to follow down after the shrinking nucleus, the *work* expended in mutual crushing and dislocation of its parts is *transformed into heat*, by which, at the places where the crushing sufficiently takes place, the material of the rock so crushed and of that adjacent to it are heated even to fusion. The access of water to such points determines volcanic eruption. Volcanic heat, therefore, is one result of the secular cooling of a terraqueous globe subject to gravitation, and needs no strange or gratuitous hypothesis as to its origin.

In order to test the validity of this view by contact with known facts, the author gives in detail two important series of experiments completed by him:—the one on the actual amount of heat capable of being developed by the crushing of sixteen different species of rocks, chosen so as to be representative of the whole series of known rock formations from Oolites down to the hardest crystalline rocks; the other, on the coefficients of total contraction between fusion and solidification, at existing mean temperature of the atmosphere, of basic and acid slags analogous to melted rocks.

The latter experiments were conducted on a very large scale; and the author points out the great errors of preceding experimenters, Bischoff and others, as to these coefficients.

By the aid of these experimental data, he is enabled to test the theory produced when compared with such facts as we possess as to the rate of present cooling of our globe, and the total annual amount of volcanic action taking place upon its surface and within its crust.

He shows, by estimates which allow an ample margin to the best data we possess as to the total annual vulcanicity of all sorts of our globe at present, that less than one fourth of the total heat at

present annually lost by our globe is upon his theory sufficient to account for it ; so that the secular cooling, small as it is, now going on is a sufficient *primum mobile,* leaving the greater portion still to be dissipated by radiation. The author then brings his views into contact with various known facts of vulcanology and seismology, showing their accordance.

He also shows that to the heat developed by partial tangential thrusts within the solid crust are due those perturbations of hypogeal increment of temperature which Hopkins has shown cannot be referred to a cooling nucleus and to differences of conductivity alone. He further shows that this view of the origin of volcanic heat is independent of any particular thickness being assigned to the earth's solid crust, or to whether there is at present a liquid fused nucleus, all that is necessary being a *hotter* nucleus than crust, so that the rate of contraction is greater for the former than the latter. The author then points out that, as the same play of tangential pressures has elevated the mountain-chains in past epochs, the nature of the forces employed sets a limit to the height of mountain possible of the materials of our globe.

That volcanic action due to the same class of forces was more energetic in past time, and is not a uniform but a decaying energy now. Lastly, he brings his views into relation with vulcanicity produced in like manner in other planets, or in our own satellite, and shows that it supplies an adequate solution of the singular and so far unexplained fact that the elevations upon our moon's surface, and the evidences of former volcanic activity, are upon a scale so vast when compared with those upon our globe.

Finally, he submits that if his view will account for all the known facts, leaving none inexplicable, and presenting no irreconcilable conditions or necessary deductions, then it should be accepted as a true picture of nature.

"On the Action of Electricity on Gases." By Sir B. C. Brodie, Bart., F.R.S., Hon. D.C.L. Oxon.

This memoir, which is intended to be the first of three communications as to the action of electricity on gases, is devoted to the consideration of the changes produced by the action of electricity on oxygen gas as estimated by the changes thus effected in its chemical properties.

The memoir is divided into four sections.

Section I. contains an account of the methods employed for generating, collecting, and preserving the electrized gas, and also of the measuring-apparatus employed for estimating the changes in the volume of the electrized gas effected in the various experiments subsequently described.

The gas, carefully dried, was submitted to the action of electricity by causing a current of the gas to pass through the induction-tube of Siemens, the interior of which was filled with water or (where a low temperature was desired) with a saline solution. The tube was placed in a glass cylinder containing water or a refrigerating-

mixture. The interior and exterior of the tube were respectively connected with the terminals of a powerful Ruhmkorff's coil. The electrized gas, after its passage through the induction-tube, was collected, in a gas-holder of peculiar construction, over concentrated sulphuric acid. It may be thus preserved for several hours without sensible variation in its properties.

The principle employed for the measurement of the gas in which it was desired to estimate the changes in volume produced by the experiment, was the principle of pipette-measurement which has been so successfully employed by chemists for the measurement of liquids. In this way a considerable volume (say from 250 to 300 cub. centims.) may be measured with facility and precision. A definite volume of gas was thus always operated upon.

The gas having been measured in the pipette was drawn over by means of a mercurial aspirator, an instrument which served the double purpose of an aspirator and measuring-apparatus. The principle of this aspirator was that originally employed in the apparatus of Regnault for measuring the volumes of gases, namely the estimation of the pressure and temperature at which the gas occupied a known space, from which the volume of the gas at standard temperature and pressure was calculated. By means of this apparatus a change in the volume of the electrized gas, to the extent of about 1 part in 1000, could be accurately estimated; that is to say, after the calibration of the apparatus, 1000 volumes of gas as measured in the pipette were found to measure 1000·7 volumes in the aspirator. These numbers represent the errors of the experiment, and any differences in the volume of the gas beyond this limit must be considered to be due to the experiment to which the gas was submitted. The pipette and the aspirator were placed on a table, separated by an interval of about 8 or 10 inches.

In Section II. the results are given of passing the electrized gas through a solution of neutral iodide of potassium, also of heating the gas, of passing the gas over metallic silver, copper, gold, aluminium, and binoxide of manganese, and of the decomposition of a solution of binoxide of sodium effected by the passage of the gas, a quantitative estimation of the changes in the volume of the gas and the oxidation effected being in all cases made.

The precision attained in such experiments may be estimated by the results of the measurement of the gas before and after its passage through a solution of neutral iodide of potassium. As the mean of eight concordant experiments, 100 cub. centims. of gas, as measured in the pipette, were found after the experiment to measure 99·93 cub. centims. in the aspirator. An oxidation was effected in the solution of neutral iodide of potassium equivalent to 3·77 cub. centims. of oxygen; that is to say, 3·77 cub. centims. of oxygen were thus removed from the gas without an appreciable variation in its volume. The volume of the gas thus absorbed by neutral iodide of potassium was in subsequent experiments assumed as the unit to which other analogous variations were referred.

When the electrized gas is passed through a solution of binoxide

of sodium, an increment occurs in the volume of the gas. Thus in two experiments the increment in the volume of the gas, as estimated from the difference of the volumes in the pipette and the aspirator, was 1·93, 1·99, the "titre" of the gas (as just explained) being taken as 1 ; and the ratio of the sum of the oxygen lost by the binoxide of sodium (as estimated by titration of the solution of binoxide of sodium before and after the experiment with permanganic acid) and the titre of the gas to the titre of the gas was in the same experiments 2·06, 2·17. In two other experiments this same ratio was 2·00, 2·08,—the reaction being analogous to the decomposition of binoxide of sodium by ferrocyanide of potassium, and of binoxide of hydrogen by permanganic acid, previously investigated by the author.

Section III. comprises an account of the action of the electrized gas upon a solution of hydriodic acid, strongly alkaline hyposulphite of soda, polysulphide of sodium, and other substances.

In the case of the passage of the gas through a solution of hydriodic acid, the oxidation effected (after a certain degree of concentration of the acid has been attained) is exactly twice the oxidation effected by the same gas in a solution of the neutral iodide of potassium. The mean of 33 such experiments gave 1·99 as the amount of oxygen employed in the oxidation of the hydriodic acid as compared with the "titre" of the gas. The individual experiments exhibit no inconsiderable differences; but the probable error of the result, as estimated by the method of least squares, is 0·02; that is to say, from these experiments alone, without introducing any hypothetical considerations whatever, it is an equal chance that the true value of the ratio sought lies between the values 2·01 and 1·97. The value indicated by chemical theory is 2, with which theory, therefore, the experiments agree.

The action of the gas upon a strongly alkaline solution of hyposulphite of soda is precisely of the same character. The volume of gas was measured before and after the experiment; and a contraction was found to occur equal in amount to the "titre" of the gas. The mean of twenty-two experiments gave 1·03 as the amount of this contraction. The peculiar oxidizing properties of the ozone are entirely destroyed by its passage through the solution; and it is to be inferred that while the diminution in volume is equal to the "titre" of the gas, the oxidation effected in this, as in the previous case, is the same as that which would be effected by a volume of oxygen equal to twice the "titre" of the gas.

Experiments made with a solution of polysulphide of barium gave a similar result.

Section IV. comprises various experiments made with solutions of neutral and slightly alkaline hyposulphite of soda, with oil of turpentine, and with protochloride of tin.

The experiments with neutral and slightly alkaline hyposulphite of soda were conducted precisely in the same manner as the experiments described in Section III. with the strongly alkaline hyposulphite. The result, however, is very different, the contraction in this case being equal in amount to twice the "titre" of the gas. The

mean of 17 experiments made with the neutral hyposulphite gave 2·02 as the value of this contraction; and the mean of 10 experiments made with the slightly alkaline hyposulphite gave for it the same value.

It is hence to be inferred that the oxidation effected in these cases is equal in amount to three times the oxidation effected by the same gas in neutral iodide of potassium.

Similar experiments made with oil of turpentine entirely confirmed the view of Soret as to the amount of contraction which the gas undergoes when acted upon by this substance, the mean of eight experiments giving 2·02 as the value of the contraction.

The investigation of the effect due to the action of the electrized gas upon protochloride of tin is attended with considerable difficulty, from the circumstance that a solution of protochloride of tin is readily oxidized by the action of pure oxygen. The difficulty was met in two ways, both of which led to the same conclusion, namely :— by applying a correction for the oxidation effected by the oxygen with which the ozone was associated; and by using very dilute solutions of the protochloride of tin, in which this oxidation is reduced to a minimum. In two experiments conducted on the latter principle, and in which the oxidation, as well as the contraction, was experimentally determined, the value of the contraction was found to be 2·19 and 2·33, while the oxidation in the two experiments respectively was 3·12 and 3·07.

Using the notation employed by the author in a previous communication* to the Royal Society, and putting ξ^2 as the symbol of the unit (that is of 1·000 cub. centim. at 0° and 760 millims.) of oxygen, and putting $[\xi]$ as the symbol of that simple weight ξ transferred to the oxidized substance in the various oxidations effected by the ozone, and further assuming that ozone is to be regarded as some denser form of oxygen, to the unit of which the symbol ξ^{2+n} (where n is a positive integer) is to be assigned, the result of the total system of experiments of which the account is given in this memoir may be expressed, so far as regards the distribution of the matter of the unit of the ozone, in the various reactions by the general equation

$$(p+q)\,\xi^{2+n} = q\xi^2 + (p(2+n)+qn)\,[\xi],$$

where p, q, n are positive integers.

The investigation of the various hypotheses originating in this equation leads to the conclusion that the hypothesis that the unit of ozone is composed of three simple weights, ξ, and is to be symbolized as ξ^3, is both necessary and sufficient for the explanation of the total system of phenomena, and that no other hypothesis of the order referred to is tenable.

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* *Vide* "Calculus of Chemical Operations," by Sir B. C. Brodie, Bart., F.R.S., Phil. Trans. 1866, pp. 781–859.

[Continued from p. 233.]

April 24, 1872.—Prof. Ramsay, F.R.S., V.P., in the Chair.

The following communications were read :—

1. "An Extract from a Despatch from H. M. Minister in Tehe-ran." Communicated by the Rt. Hon. the Earl Granville, Secretary of State for Foreign Affairs.

This letter described the effects of some severe earthquake shocks experienced at Khabooshan in North-Western Khorassan. On the 23rd December, 1871, an earthquake occurred which destroyed half the town of Khabooshan, and buried about 2000 of its inhabitants in the ruins. On the 6th January, 1872, another severe shock de-stroyed the remainder of the town, and killed about 4000 people. Four forts near the town were so completely buried that not a trace of them can be seen. It was estimated that 30,000 lives were lost in Khabooshan, Bojnoord, and the surrounding villages by the effects of these earthquakes.

2. "Notes on the Geology of the Colony of Queensland." By R. Daintree, Esq., F.G.S.

The author stated that ALLUVIAL DEPOSITS are very scanty in Queensland, except on the northern shores of Carpentaria and near the mouths of the larger rivers. The fossil remains of extinct Mammalia (*Diprotodon, Macropus, Thylacoleo, Nototherium,* &c.) are found in old brecciated alluvia, representing beds of old water-courses, through which modern creeks have cut their channels. With these mammalia are found shells of existing species.

Of CAINOZOIC DEPOSITS the most important is called the "Desert Sandstone" by the author; it consists of horizontal beds of coarse grit and conglomerate, nowhere exceeding 400 feet in thickness, forming a sandy barren soil by their disintegration. The only fossils found in it are rolled fragments of coniferous wood; and its strati-graphical position is determined solely by its resting unconformably upon beds containing apparently Cretaceous fossils. The author considered that this deposit formerly covered nearly the whole of Australia.

Beds containing MESOZOIC forms of fossils, and referred by the author to the Cretaceous series, occur upon the Upper Flinders. At Marathon these deposits consist of a fine-grained yellow sand-stone, and below this a series of sandstones and argillaceous lime-stones, containing four species of *Inoceramus*, with a species of *Ichthyosaurus* and two of *Plesiosaurus*. At Hughenden station, near Mount Walker, there is a series of calcareo-argillaceous beds, probably inferior to those of Marathon, and containing two species of Ammo-nites, with *Avicula gryphæoides*, a *Pecten*, &c. At Hughenden cattle-station, twenty miles further up the river, numerous Belem-nites are found loose upon the surface. These Mesozoic rocks also extend down the Thompson River and its tributaries. The author referred to the fossils described by Mr. Charles Moore as probably Oolitic, and stated that it is more than probable that Oolitic and

Cretaceous rocks extend throughout the whole of Central Queensland, and thence to Western Australia. On the eastern side of the dividing range a small patch of ferruginous grit containing *Panopœa plicata*, occurs near Pelican Creek; and from Gordon Downs species of *Panopœa*, *Pholadomya*, and *Cucullœa* have been obtained. These beds probably represent a lower horizon than those on the Flinders River; and a large portion of the colony east of the dividing range is covered by freshwater deposits, containing plant-remains (including *Tæniopteris*), and in their upper part a fauna apparently intermediate between the Gordon-Downs and Flinders-River series. In these deposits, on the Condamine, Brisbane, and Mary rivers, numerous Coal-seams exist. The author supposes that, contemporaneously with the deposition of a series of marine beds to the west of the dividing range, during the Oolitic and part of the Cretaceous period, a vast lacustrine deposit was accumulated over a large area to the eastward of the range, to which the sea subsequently obtained access.

Among the Palæozoic deposits, the author distinguished Carboniferous and Devonian rocks. The *Carboniferous* series was said to be represented in northern Queensland by an extensive Coalfield. The upper portion of the series (grits, sandstones, and shales) contains chiefly fossil plants, the most abundant being a *Glossopteris*. The lower strata (generally *argillaceous* limestone) contain *Producti*, *Spiriferæ*, &c. of true Carboniferous type, intermixed with scanty and imperfect remains of the above-mentioned plants. A set of fossils from the head of the Don River were said to agree with those found in the Hunter-River series of New South Wales.

Devonian rocks extend from 18° S. lat. to the southern boundary of Queensland and for 200 miles inland. They consist of slates, sandstones, and Coral-limestones. The upper portion of this series contains an abundance of fossil plants, the deposits containing which, at Mount Wyatt, are interstratified with beds containing *Spiriferæ*; and other fossils of Devonian type occur in beds reached by shafts sunk through these strata. In the limestone of the lower portion of the series corals are very numerous. On the Broken River this formation may be best studied. Gold is found in many parts of the Devonian district; and the author entered in considerable detail into its mode of occurrence there.

Metamorphic rocks were described by the author as occurring in various localities. At the Cloncurry, Cape-River, Gilbert, Peak-Downs, Black-Snake, Kilkwan, and Goaroomjain Diggings these are mica- and hornblende-schists, whilst at the Ravenswood Diggings the rock is a granite with triclinic felspar. The latter, which contains more or less hornblende, the author regarded as of metamorphic origin. The author noticed the connexion between the presence of certain trappean rocks in these metamorphic areas and in the Devonian area and the production of auriferous and cupriferous lodes.

True *Granites* crop out along the eastern coast of Queensland; and these vary much, passing into porphyry and quartz-porphyry; but monoclinic felspar always predominates in them.

The intrusive TRAPPEAN rocks, which are regarded as influencing the production of auriferous veinstones in the Devonian and metamorphic rocks, are noticed at considerable length by the author, and consist of pyritous porphyrites and porphyries, pyritous diorites and diabases, chrome-iron serpentines and pyritous felsites; the author considers that this order probably indicates the succession of these rocks in time. The veinstones he thinks were probably deposits of mineral matter from the hydrothermal action which preceded, accompanied, and continued long after the cooling of the traps themselves.

The VOLCANIC ROCKS, in the author's opinion, have played a most important part in determining the elevation and present physical outline of North-eastern Queensland; they follow the line of greatest elevation on the main watershed at altitudes of from 1500 to 2000 feet above the sea-level. The general arrangement of the other rocks referred to is epitomized by the author as follows :—

"With the exception of the McKinlay ranges, a line drawn parallel with the eastern coast at a distance of 250 miles would include all the Palæozoic, metamorphic, granitic, Trappean, and volcanic rocks represented in the colony, both coal-groups lying within the same area.

"The Mesozoic and Cainozoic systems occupy the surface-area to the westward.

"The descent going eastward is first locally a thin capping of 'Desert Sandstone,' next Carboniferous, then Devonian and possibly Silurian, with patches of metamorphic and granitic rocks interspersed.

"The chief granitic mass extends from Broad Sound to Cape York, with an occasional capping of 'Desert Sandstone.'"

The paper contained numerous analyses of the various rocks; and the fossils have been worked out by Messrs. Etheridge and Carruthers, whose lists and descriptions of them are appended to the paper.

LVIII. *Intelligence and Miscellaneous Articles.*

ON THE COLLISION OF ELASTIC BODIES, AND A NUMERICAL VALUATION OF ITS DURATION. BY H. SCHNEEBELI [*].

COLLISION has long been studied and in divers manners, but always with respect to its influence on the bodies which take part in it, and not with reference to the duration of their contact; this has hitherto been entirely neglected, it having been taken for granted that the duration was excessively small and difficult to measure. It is precisely the study of this question to which M. Schneebeli has devoted himself. He has applied a method devised, for this kind of researches, by Pouillet, and which consists in profiting by the contact to close a galvanic circuit, and deducing its duration from the galvanometric deflection produced by a known current which traverses the circuit during the whole time of contact.

For this purpose the galvanometer must previously be calibrated—

that is to say, the relation established which exists between the duration of the current, supposed constant, and the deflection it produces upon the galvanometer-needle. For this purpose M.Pouillet made use of a contact-breaker consisting of a glass disk carrying on one of its faces a strip of metal arranged as a radius. In its rotation the metal strip rubbed lightly against a metal spring. One of the poles of the pile being connected with the strip, the other with the spring, from the velocity of rotation of the disk and the distance of the spring from the axis the duration of the current could be deduced and the galvanometer-deflections corresponding to different durations could be measured. M. Schneebeli substituted for this another arrangement, consisting of a metallic pendulum carrying at its lower part a triple spring ; this rubbed against a horizontal strip of steel fixed in the same vertical plane as the axis of rotation of the pendulum. That there might be no shock at the moment of contact, a glass plate applied horizontally to the steel on the same side as the approaching pendulum forced the spring to bend gradually. The pendulum was connected with one of the poles of the pile, and the strip with the other ; and the Meyerstein galvanometer (with mirror and rule) was introduced into the circuit, which was traversed by the current from one or two very clean Bunsen pairs. The duration of contact in this apparatus was inversely proportional to the square root of the height of the fall H of the pendulum.

Now, on constructing the curve of which the values of $\dfrac{1}{\sqrt{H}}$ were the abscissæ, and the galvanometer-deflections the ordinates, the author obtained a right line passing very distinctly through the origin ; from which it follows that *the deflection of the galvanometer-needle is proportional to the duration of the current which produces it.*

The intervals of time during which the current was allowed to act varied between 0·00015 and 0·00070 of a second. It being so, M. Schneebeli studied by means of this method the conditions upon which the more or less prolonged duration of the shock of two elastic bodies depends. He operated upon one substance only, and commenced with a simple case, that of impact on a plane surface. This was the upper base, quite flat and smooth, of a right cylinder of steel 2 metres in length and of 36 millims. diameter, firmly fixed, and connected with the galvanometer. The impact was always direct and central. The striking body, a ball or a cylinder, was attached to a conducting wire which put it in communication with the pile and, besides, with the galvanometer. The current passing during the time the contact continued, gave, in the galvanometer-deflection, a relative measure of the duration of the impact. The author had previously satisfied himself that the form and relative dimensions of the two surfaces exerted no sensible influence on the conductivity of the circuit, and consequently on the deflection.

1. *Influence of the mass of the striking body on the duration of the collision.*—On letting fall on the plane surface of steel some cylinders also of steel, of the same length but of different diameters, and all having their lower extremity identically spherical, it was ascertained

that *the duration of collision increases with the mass of the striking body and almost proportionally to that mass.*

2. *Influence of the height of fall of the striking body.*—The experiments were made with balls of steel, and showed that, *when the height of fall is increased, the duration of the collision is diminished.*

3. *Influence of the radius of curvature of the striking body.*—Four cylinders, let fall from the same height upon the plane, were of the same dimensions and the same mass; but their lower extremities were spherical caps of different radii; and it was demonstrated that *the duration of collision diminishes when the radius of curvature of the striking body increases.*

4. *Influence of the length of the striking body.*—With cylinders of the same weight, terminated by identical spherical caps, but of different lengths, the author found that *the duration of collision augments with the length of the striking body.*

5. *Collision between two identical balls.*—Here the author distinguishes two cases:—that in which the ball struck is suspended freely; and that in which it is fixed to the vertical face, on this occasion, of the steel cylinder.

Calling *a* the duration of the impact of the first ball against the steel cylinder, *b* the duration of the impact of the second ball against the other when it rests against the cylinder, and *c* the duration of the impact of the second ball against the first freely suspended, we have
$$b = 3a = 4c.$$

6. *Collision between bodies of different dimensions suspended freely.*—Of two balls of different dimensions, the author caused now the small ball to strike against the large one originally motionless, then the large one against the small—or, again, a ball against the large cylinder freely suspended, then the cylinder against the ball; and he ascertained that, *in the collision of two elastic bodies, it is immaterial for the duration of the contact whether it is the larger or the smaller of the two which strikes the other.*

7. *Numerical valuation of the duration of collision.*—The duration of the collision of two elastic bodies is always very short. To give an idea of it, M. Schneebeli cites a valuation he made of it in a particular case: a steel cylinder of 695 grammes weight, falling from a height of 33 millims. upon the face of the large steel cylinder, remains in contact with it during a time
$$t = 0 \cdot 00019 \text{ second.}$$
—*Bibl. Univ., Arch. des Sciences,* vol. xliv. pp. 332–335.

SPECTRUM OF THE AURORA. BY EDWARD I. HOLDEN, SECOND LIEUTENANT OF ENGINEERS.

I have this evening succeeded in observing the spectrum of a very fine aurora, which appeared about 7 P.M., and lasted perhaps 20 minutes. It first appeared as a rosy cloud about 15° wide and perhaps 30° high, bearing N. 30° W. by compass. Afterwards it spread to the zenith, and was principally in the shape of a band

(say) 15° wide, extending from the N.W. to the E.   No pulsations of any magnitude were evident; but a radiated structure was manifest.

The spectroscope (pocket, by Hawkins and Wales) was first turned on the full moon, and an idea of the length of the spectrum obtained; then with a wide slit it was turned on the aurora, and the following sketch made, which was *carefully verified,* so that it represents exactly what I saw.

The length M N is what I conceived to be the length of the spectrum given by my instrument under usual conditions.   The violet (extreme) rays seemed cut off; and I saw, 1st, a broad and bright *red* band (R), 2nd, a black space equal in width to it (B), 3rd, a green and bright band (G) nearly as wide, then a faint spectrum of diffused light, and a bright line in the blue (1), then a bright line more refrangible but whose colour could not be definitely seen (2).   The relative distances for my instrument are kept in the drawing.   I then opened Ångström's 'Spectre Normal,' and saw that he gave the auroral line as in the yellow.   I observed this green line again, and cannot persuade myself that it was yellow.   The black space I am *sure* of; and it was also seen plainly by an inexperienced person into whose hands I put the instrument.   The slit was then narrowed and turned on the moon, and adjusted to give the Fraunhofer lines most clearly. The aurora by this time was fainter, and I can only be sure of a bright line (green) with a suspicion of my former blue line.   Opening the slit again, the red band of the diffused-light spectrum was *close* against the green bright line.   The aurora then faded.   I mention this black space, as it is not what I expected to see from my reading of Ångström and Winlock.—Silliman's *American Journal,* Nov. 1872.

CONTINUATION OF THE OBSERVATIONS RELATIVE TO THE PRESENCE OF MAGNESIUM IN THE CHROMOSPHERE OF THE SUN.

BY M. TACCHINI.   Palermo, July 30, 1872.

As a sequel to my previous note on this subject[*], I take leave to give some details on the subsequent observations.   The frequency of magnesium has been considerable till this morning, but with very pronounced maxima and minima.   It would be impossible for me at present to make a complete report, which would require much time; I shall confine myself to the truly extraordinary last period, that from the 25th to the 30th of July.   The following are the numbers of degrees which express the distances, referred to the margin, of the points where the magnesium was visible:—

July 25 ...... 176°	July 28 ...... 348°
,, 26 348	,, 29 348
,, 27 348	,, 30 258

* Phil. Mag. August 1872, p. 159.

We have therefore four days on which the phenomenon was complete ; for only two positions of the spectroscope are wanting.   As I remarked in the preceding note, the magnesium-lines appeared brighter and broader in the regions where the flames of the chromosphere were more pronounced and brilliant ; but I must now say that the chromosphere is rather composed not of flames, but of an extraordinary number of slender threads which resemble very fine hair.   In the vicinity of the north pole the chromosphere continues to be much more elevated than at the south pole.

Here is another observation which agrees with the great abundance of magnesium, and which I had the opportunity of making only yesterday :—When observing the lines b, I can see at the same time a portion of the spectrum beyond the line E.   After exploring about one third of the margin, I perceived the reversal, in two successive positions, of Kirchhoff's line 1474 ; I then recommenced the tour, and found the line 1474 reversed all round the margin in the chromosphere, like the magnesium ; and I verified the same phenomenon again today : therefore the line of the corona can be distinguished everywhere in full sunshine.   In the preceding months I had observed this line many times, but only in the special examination of the spectrum of the protuberances or of the characteristic lines of the margin.

The relative intensity of the line 1474 agrees generally with that of the magnesium, which corresponds with the number of reversed lines of the group b.   In other terms, when the magnesium-line is faint, we see only the line b^1 reversed ; and when it was very bright, I could see all the lines b^1, b^2, b^3, b^4 reversed.

To give an example of the distribution of these lines on the margin, I here relate the observations made in each position, on the 29th of July, 1872, between 9 and 10 A.M. :—

N. 0°		1474	W. 90°		1474	S. 180°		1474	E. 270°		1474
0	$b^1b^2b^3$	1474	90	$b^1b^2b^3b^4$	1474	180	b^1	1474	270	$b^1b^2b^3$	1474
6	$b^1b^2b^3$	1474	96	$b^1b^2b^3b^4$	1474	186	$b^1b^2b^3$	1474	276	b^1b^2	1474
12	$b^1b^2b^3$	1474	102	$b^1b^2b^3$	1474	192	$b^1b^2b^3$	1474	282	b^1b^2	1474
18	$b^1b^2b^3$	1474	108	$b^1b^2b^3$	1474	198	b^1b^2	1474	288		
24	$b^1b^2b^3$	1474	114	$b^1b^2b^3$	1474	204	b^1b^2	1474	294	b^1	1474
30	$b^1b^2b^3$	1474	120	$b^1b^2b^3$	1474	210	b^1b^2	1474	300	b^1	1474
36	$b^1b^2b^3$	1474	126	b^1b^2	1474	216	b^1b^2	1474	306	b^1	1474
42	$b^1b^2b^3$	1474	132	$b^1b^2b^3$	1474	222	$b^1b^2b^3$	1474	312	b^1	1474
48	$b^1b^2b^3$	1474	138	$b^1b^2b^3$	1474	228	b^1b^2	1474	318	b^1b^2	1475
54	$b^1b^2b^3$	1474	144	b^1b^2	1474	234	b^1	1474	324	b^1b^2	1474
60	$b^1b^2b^3b^4$	1474	150	$b\ b^2b^3$	1474	240	b^1	1474	330	b^1b^2	1444
66	$b^1b^2b^3b^4$	1444	156	b^1b^2	1474	246	b^1b^2	1474	336	b^1b^2	1474
72	$b^1b^2b^3b^4$	1474	162	b^1b^2	1474	252	$b^1b^2b^3$	1474	342	b^1b^2	1474
78	$b^1b^2b^3$	1474	168	......	1474	258	b^1	1474	348	$b^1b^2b^3$	1474
84	$b^1b^2b^3b^4$	1474	174	b^1b^2	1474	264	b^1b^2	1474	354	$b^1b^2b^3$	1474

From the 19th to the 31st of July I observed very brilliant spectra in some portions of the margin completely destitute of protuberances, but composed of small bright flames and corresponding to numerous minute faculæ, without any appearance of eruption.— *Comptes Rendus de l'Acad. des Sciences,* August 12, 1872, pp. 430, 431.

LIX. *On the Heat-conducting Power of Iron and German Silver. By* H. WEBER *of Brunswick*.*

[With a Plate.]

WITH the exception of the insufficient experiments of Péclet, a determination of the heat-conducting power of different metals was first accomplished not long since, namely by F. Neumann, Ångström, and Forbes. The values found by them exhibit not inconsiderable divergencies from one another, which cannot be referred *only* to the difference of the sorts of metal employed. It appears that they are much rather to be in great part attributed to the fact that the phenomena, from the observation of which the conducting-power was deduced, were very complicated, whence great difficulties arose. Besides, it can be proved that the theory made use of for the calculation of the observations, strictly taken, has only an approximative value. Indeed neither Fourier's nor Poisson's theory takes account of the heat expended for the expansion, and of the alterations of the specific heat and density with the temperature. Moreover the assumption which forms the basis of Fourier's whole theory has been called in question—viz. that the quantity of heat passing in the unit of time through a plate which is in a stationary condition is proportional *solely to the difference of temperatures* of the boundary surfaces; probably it is dependent also upon the *absolute temperature* of the plate. Lastly, almost greater objections can be raised against the employment of Newton's law of cooling. Nevertheless the results derived from accurately conducted observations might still possess an approximative value for many important purposes, in particular for many problems most intimately connected with the mechanical theory of heat; for, to obtain only a nearer insight into the phenomena of heat, such approximative results are often quite indispensable.

Hence, in the determination of heat-conducting power, attention must be directed especially to the *simplicity of the phenomena* from the observation of which it is to be deduced. Now this simplicity appears to have been attained in a far higher degree by a method which F. Neumann has given in his Lectures than by any other hitherto employed, even in a higher degree than by the one made use of by Ångström (described in Pogg. *Ann.* vol. cxiv. p. 513), to which Neumann's stands in the closest relation.

The following observations, made according to this method with the greatest care possible, for an exact determination both of the *internal and the external conducting-power of iron and German silver*, have fully established the superiority here ascribed to it.

Determination of the Heat-conducting Power after Neumann's Method.

Ångström used in his experiments a bar of everywhere equal cross section, and of such a length that the temperature of one end did not perceptibly vary when the other end was exposed to different high temperatures. The one end was now, at equal intervals of time, alternately brought to two different high temperatures, and thereby a periodical state of temperatures produced in the bar, from the observation of which the value of the internal conducting-power was finally obtained. In order to deduce the mathematical expression for that state of the bar, Ångström represented the temperature-state of the alternately heated and cooled end, by means of a series of sinuses, as a function of the time. If we denote the same by $\phi(t)$, by u_0 and u_1 the temperatures which the end-surface alternately assumes, by T the interval of time after which each change occurs, then

$$\phi(t) = \frac{u_0 + u_1}{2} + \frac{u_0 - u_1}{2} \frac{4}{\pi} \left\{ \sin\frac{\pi}{\mathrm{T}} t + \frac{1}{3} \sin\frac{3\pi}{\mathrm{T}} t + \dots \right\}.$$

Neumann has now shown that the same problem can be treated in another way, and one more favourable for observation, —and that it is more suitable for the determination of the two conducting-powers to subject not merely *one* end of a bar regarded as unlimited, but *both* ends of a bar of infinite length to the same periodic change of temperature—in such manner that when the two ends in the 0th period have the temperatures u_0 and u_1 they in the next following take the temperatures u_1 and u_0, in the 2nd period (on the contrary) they are in the same state as in the 0th, and in the 3rd as in the 1st, &c.

In order to determine the temperatures which are hereby produced in the different parts of the bar after the lapse of a series of periods, we have first to determine the temperatures
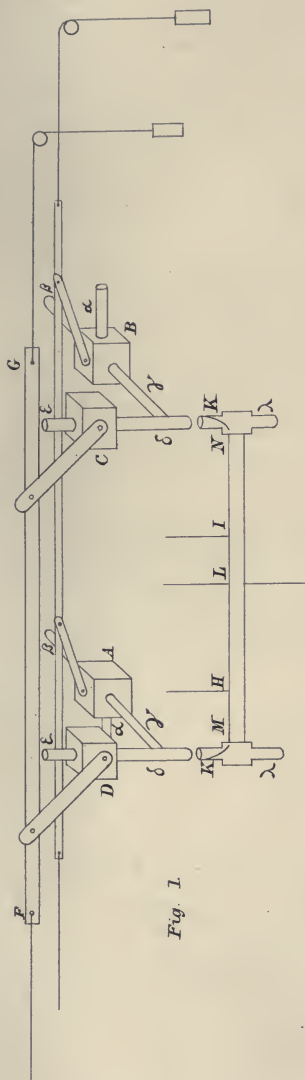
Fig. 6.

Fig. 1.
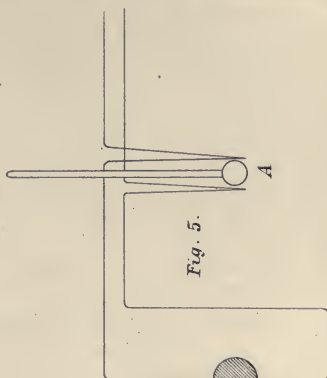
Fig. 5.

Fig. 4.
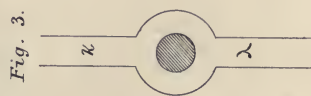
Fig. 3.
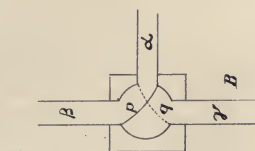
Fig. 2.
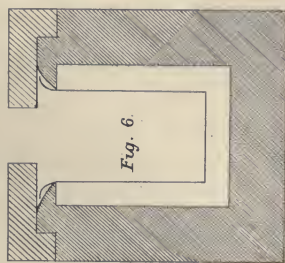
assumed by the bar in any initial condition when its ends are suddenly brought to the temperatures u_0 and u_1. Hence we obtain the state of the bar at the end of the 0th period. We have to solve the same problem for the 1st period, in which the ends have the temperatures u_1 and u_0, taking as initial state that of the bar at the close of the 0th period. From this we find the state of the bar at the close of the 1st period, which forms the initial state of the next following period. Proceeding in this way we may easily satisfy ourselves that the distribution of temperatures in the bar very soon approaches two limiting states, which continually repeat themselves, and one of which belongs to the even, the other to the odd periods. Both of these limiting states are absolutely independent of the arbitrary distribution of temperatures at the commencement of the 0th period.

We will now give the result of these considerations, and for this purpose introduce the following symbols:—Let

x be the distance of any point in the bar, reckoned from that end which in the 0th period possessed the temperature u_0,

T the duration of a period,

θ the time within the current period ($\theta = 0$ commences the period; $\theta = T$ closes it),

u_0 and u_1 the alternate temperatures of the end surfaces of the bar,

$V_{2\mu}$ and $V_{2\mu+1}$ the temperatures of any point at the time θ in an even and in an odd period respectively,

l the length of the bar,

U the temperature of the surrounding air,

K and H the internal and the external conducting-power,

P and Q the circumference and the transverse section of the bar,

C the specific heat,

D the density.

Lastly, let us put (for abbreviation)

$$k = \frac{K}{CD}, \quad h = \frac{HP}{QCD}. \quad \cdot \quad \cdot \quad \cdot \quad \cdot \quad (1)$$

Then is

$$\left.\begin{aligned}
V_{2\mu} &= U + \left(\frac{u_0 + u_1}{2} - u\right)(Z_0 + Z_1) - \frac{u_0 - u_1}{2}(Z_0 - Z_1) \\
&\quad - \frac{8\pi k}{l^2}(u_0 - u_1)\Sigma_1^\infty \frac{n}{p_n}\sin\frac{2n\pi}{l}x\frac{e^{-p_n\theta}}{1 + e^{-p_nT}}, \\
V_{2\mu+1} &= U + \left(\frac{u_0 + u_1}{2} - U\right)(Z_0 + Z_1) + \frac{u_0 - u_1}{2}(Z_0 - Z_1) \\
&\quad + \frac{8\pi k}{l^2}(u_0 - u_1)\Sigma_1^\infty \frac{n}{p_n}\sin\frac{2n\pi}{l}x\frac{e^{-p_n\theta}}{1 + e^{-p_nT}},
\end{aligned}\right\} \quad (2)$$

in which (for shortness) we have put

$$Z_0 = \frac{e^{x\sqrt{\frac{h}{k}}} - e^{-x\sqrt{\frac{h}{k}}}}{e^{l\sqrt{\frac{h}{k}}} - e^{-l\sqrt{\frac{h}{k}}}} \quad Z_1 = \frac{e^{(l-x)\sqrt{\frac{h}{k}}} - e^{(l-a)\sqrt{\frac{h}{k}}}}{-e^{l\sqrt{\frac{h}{k}}} - e^{l\sqrt{\frac{h}{k}}}}$$

$$p_n = \frac{4k\pi^2}{l^2} n^2 + h.$$

If in the expression for $V_{2\mu}$ we suppose $\theta = T$, while for $V_{2\mu+1}$ it is supposed $= 0$, the two expressions will be equal to one another. It is the same when $\theta = T$ in the expression for $V_{2\mu+1}$ while $\theta = 0$ in that for $V_{2\mu}$; and hence, in fact, the final state of the bar in one period is the initial state of the following*.

It follows from the above expressions that the temperature in the middle of the bar remains constant during each period, and preserves the same value in the even and the odd periods. That is to say, for $x = \dfrac{l}{2}$ we obtain from the equations (2), for each of the two periods, if V_m denotes the temperature of the middle of the bar :—

$$V_m = U + \left(\frac{u_0 + u_1}{2} - U\right) \frac{2}{e^{\frac{1}{2}l\sqrt{\frac{h}{k}}} + e^{-\frac{1}{2}l\sqrt{\frac{h}{k}}}}.$$

By observing V_m, and U, u_0, and u_1 being given, the ratio $\dfrac{h}{k}$ can very easily be deduced; for if we put, for shortness,

$$\frac{\dfrac{u_0 + u_1}{2} - U}{V_m - U} = d,$$

then

$$\frac{h}{k} = \frac{4}{l^2} \left[\frac{\log d + \sqrt{d^2 - 1}}{\log e}\right]^2. \quad \cdot \quad \cdot \quad (3)$$

In this we have *one* equation between the quantities h and k; it remains to construct a *second* between the same. Such an equation can be derived in various ways from the equations (2). On closer consideration, however, we find that it is far the most

---

* We may easily satisfy ourselves of this by employing the series

$$\frac{8\pi k}{l^2} \Sigma_1^\infty \frac{n}{p_n} \sin\frac{2n\pi}{l} x = -(Z_0 - Z_2),$$

which is convergent for all values of x between 0 and l.

convenient to deduce from equations (2) the difference of temperature of two points in the bar, one of which has the distance $x = \frac{1}{6}l$, and the other the distance $x = \frac{4}{6}l$; for in this special case all terms in which n is divisible by 2 and 3 vanish from the series which occur in both expressions; and these hereby take a form so convergent that even the second term becomes vanishingly small in comparison with the first as soon as the time θ has reached a certain value. When, therefore, a commencement is made with the observations first at the expiration of a certain time θ reckoned forward from the beginning of each period, only the first term of that series need be taken into consideration.

Let $D_{2\mu}$ denote the difference of temperature, in an even period, between two points whose distances from the end are $x = \frac{1}{6}l$ and $x = \frac{4}{6}l$, and let $D_{2\mu+1}$ denote the same value in an odd period; then, from equations (2), we find :—

$$
\left.
\begin{aligned}
D_{2\mu} &= \left(\frac{u_0 + u_1}{2} - U\right)(A - B) + \frac{u_0 - u_1}{2}(A + B) \\
&\quad - \frac{8\pi k \sqrt{3}}{pl^2} \frac{u_0 - u_1}{1 + e^{-pT}} e^{-p\theta}, \\
D_{2\mu+1} &= \left(\frac{u_0 + u_1}{2} - U\right)(A - B) - \frac{u_0 - u_1}{2}(A + B) \\
&\quad + \frac{8\pi k \sqrt{3}}{pl^2} \frac{u_0 - u_1}{1 + e^{-pT}} e^{-p\theta}.
\end{aligned}
\right\} \quad . \quad (4)
$$

Here

$$p = \frac{4\pi^2}{l^2} k + h. \quad . \quad . \quad . \quad . \quad . \quad (5)$$

$$A = \frac{\left(e^{\frac{5}{6}l\sqrt{\frac{h}{k}}} - e^{-\frac{5}{6}l\sqrt{\frac{h}{k}}}\right) - \left(e^{\frac{2}{6}l\sqrt{\frac{h}{k}}} - e^{-\frac{2}{6}l\sqrt{\frac{h}{k}}}\right)}{e^{l\sqrt{\frac{h}{k}}} - s^{-l\sqrt{\frac{h}{k}}}},$$

$$B = - \frac{\left(e^{\frac{1}{6}l\sqrt{\frac{h}{k}}} - e^{-\frac{1}{6}l\sqrt{\frac{h}{k}}}\right) - \left(e^{\frac{4}{6}l\sqrt{\frac{h}{k}}} - e^{-\frac{4}{6}l\sqrt{\frac{h}{k}}}\right)}{e^{l\sqrt{\frac{h}{k}}} - e^{-l\sqrt{\frac{h}{k}}}}.$$

The first two terms in the above expressions (4) are constant quantities; the same holds for the factor of $e^{-p\theta}$; consequently the expressions of $D_{2\mu}$ and $D_{2\mu+1}$ have the form

$$
\left.
\begin{aligned}
D_{2\mu} &= M - N e^{-p\theta}, \\
D_{2\mu+1} &= -M' + N e^{-p}
\end{aligned}
\right\} \quad . \quad . \quad . \quad (6)
$$

If, then, for a series of different values of θ in the same period we obtain by observation the temperature-differences $D_{2\mu}$ or $D_{2\mu+1}$ belonging to these times, from these we get a series of equations from which M and N or M' and N' can be eliminated; and therewith the quantity P is given. According to (5) a second equation is hereby obtained between h and k, which, in conjunction with equation (3), leads to the knowledge of h and k, and therewith also to that of H and K*.

Description of the Apparatus.

The arrangements made use of in order to fulfil rigorously the conditions presupposed in the theory were the following. The heating and cooling of the ends of the bar were effected by alternately conducting over the ends steam of about one atmosphere tension and water at a certain temperature. The bringing of steam or water took place through the four cocks, A, B, C, D (Plate V. fig. 1). The cocks A and B served for bringing the steam. Each of them had three small pipes attached, α, β, γ, which brought and carried away the steam. From two boilers, in which the steam was generated, it was conducted through pipes to the pipes α, whence, according to the position of the cocks, it either entered through γ into the pipes δ, and through these was conducted over the ends of the bar M N, or passed through the pipes β into other pipes, through which it issued into the air. Thus, during the observations, two independent currents of steam were continuously kept up; and by the arrangement described steam could at any moment, by a suitable position of the cock-plugs, be conducted to the ends of the bar. Fig 2 is a horizontal section of the cocks A and B. The line p gives one, and the dotted line q the other normal position of the plug. C and D are usually single-bored cocks: when they are in one normal position, the pipes ϵ and δ are in communication; when, on the contrary, they are in the second normal position, the pipes ϵ are shut off. The pipes ϵ were connected with the municipal waterworks. The small receivers (figs. 3 and 4), finally, into which the ends of the bar were soldered, were manufactured of very thin sheet brass. On the inside of the upper

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* If not satisfied with the first term of the above-mentioned series, we might also take the second term into consideration. For this we have

$$\frac{40k\pi\sqrt{3}}{l^2}, \quad \frac{u_0-u_1}{\frac{100\pi^2}{l^2}k+h}, \quad \frac{e-\left[\frac{100\pi^2}{l^2}k+h\right]\theta}{1+e-\left[\frac{100\pi^2}{l^2}k+h\right]\tau},$$

which would have to be added to the above expression for $D_{2\mu}$ with plus, and to the expression for $D_{2\mu+1}$ with minus signs. Its value, however, in the following observations does not amount to the millionth part of that of the first term.

pipes k a scoop-like piece of sheet brass was soldered for the purpose of diverting the entering water to the end-surface of the bar. During the experiments the ends k were connected with the pipes δ, and the ends λ with pipes which carried off the steam or water, by means of caoutchouc tubing.

It follows from the theory above given that the most favourable circumstances for the determination of K occur when the ends M and N of the rod have simultaneously different temperatures—M being heated and N cooled during the first period, M cooled and N heated during the following period, or *vice versâ*.

In order to regulate with facility the supply of the water and steam according to this arrangement, the levers which turned the plugs of the watercocks C and D, on the one hand, and those of the steamcocks A and B, on the other, were connected by a bar; so that by the motion of the two bars the four plugs could be put into their right positions. All the cocks were provided with stops, and consequently could only turn within their normal positions. With such an arrangement, it is easy to see how the four cocks could be regulated at a distance. It was only necessary to fasten cords to the ends of the connecting bars, two of which were stretched by weights, whereby the plugs took one of their normal positions; and when the cords at the other ends were pulled, the plugs were turned so as to take their second position. In order to give the cocks a firm support, each two of them (A and D, and C and B) were let into a piece of wood, by which all radiation of heat to the bar was avoided.

Instead of four cocks, two four-way cocks might have sufficed, as used by Angström in his researches. But then such a cock would have been traversed at the same time by steam and by water; and, on the one hand, steam would have been condensed, while, on the other, the temperature of the water would have been raised. By the above arrangement this inconvenience was avoided. The temperature of the water was measured by a thermometer inserted in the water-pipe.

In order to observe the distribution of temperature in the bar, in the first place a thin iron wire and a thin German-silver wire were soldered in the middle of it, L, opposite to each other. The ends of these two wires were soldered to two copper wires which led to a galvanometer, and formed with the copper wires points as shown in fig. 5. Between the points was the bulb of a sensitive thermometer. The thermometer and wires were retained in this position by a suitable keeper made of wood. If now the middle of the bar and the points (which are in the surrounding air) have different temperatures, a thermo-current ensues, the intensity of which may be supposed proportional to the difference between the temperature of the middle of the bar and that of the

points. In the places H and I, at the distances $\frac{1}{6}l$ and $\frac{4}{6}l$ from the end of the bar, two German-silver wires were soldered to the iron bar, two iron wires to the German-silver one; and their ends, at about 6 inches from the bar, were soldered to two copper wires which led to the same galvanometer as the wires before mentioned. In front of the galvanometer was a regulator such that now the conduction to the middle of the bar, and now the conduction to the places H and I could be connected with the galvanometer. Each of the thermo-currents circulating in the two conductions could be passed in opposite directions through the galvanometer; and, finally, it was also possible to shut off the galvanometer and to determine the stationary position of the needle at any moment. If the conduction to H and I was connected with the galvanometer, the intensity of the thermo-current circulating in this circuit was evidently proportional to the difference between the temperatures of H and I. The bar and the cocks were in a room by themselves, the galvanometer and the two steam-boilers were in two adjoining rooms. The pipes for steam and water were carried through the dividing wall, and wrapped tightly round with strips of cloth and tow. In this manner any considerable alteration of temperature of the air surrounding the bar during the experiments was avoided.

The Galvanometer and its Employment.

The galvanometer made use of in the experiments was constructed according to the rules for attaining the maximum of sensitiveness[*]. The length of the needle was 100 millims.; and the multiplier had a resistance of $0.59064 \times 10^{10} \dfrac{\text{millimetres}}{\text{second}}$ at a temperature of 10° C. When the galvanometer was shut off, the arresting-power was so great that the needle returned from deflections of 12° to the position of rest after two oscillations. Hence the logarithmic decrement λ could not be determined immediately from the diminution of the arc of oscillation, but had to be deduced from a series of observations in which different known resistances were inserted. For it the following value was found:—

$$\lambda = 4.2458.$$

The time of an oscillation with the circuit open was $4''.466$.

Now, for all galvanometers constructed according to the directions above mentioned, the torsion-moment can be determined which is exerted by the multiplier upon the needle deflected to an angle ϕ from the plane of the meridian when a current of the intensity I flows through the multiplier. For, denoting this

[*] Pogg. *Ann*. vol. cxxxvii. p. 121.

moment, which is called the *galvanometer-function*, by $f(\phi)$, it can be shown that

$$f(\phi) = f(0)\cos\phi(1 + a\sin\phi^2 + b\sin\phi^4 + c\sin\phi^6 + \ldots),$$

where a, b, c, ... are constants. The smaller the angle ϕ, the more rapidly convergent is this series; and in observations in which ϕ remains always small, we may content ourselves with the first two terms, so that we have

$$f(\phi) = f(0)\cos\phi(1 + a\sin\phi^2)^*. \quad \cdots \quad (7)$$

If we connect the galvanometer with the conduction to the middle of the bar, the thermo-current (whose intensity may be i) deflects the needle from the plane of the meridian to an angle ϕ; and for this new position of rest the following equation holds :—

$$i f(\phi) = \mathrm{T}m\sin\phi,$$

m being the magnetic moment, T the horizontal component of the earth's magnetism. With the aid of equation (7) we find from this :—

$$i = \frac{\mathrm{T}m}{f(0)}\frac{\tan\phi}{1 + a\sin\phi^2}. \quad \cdots \quad (8)$$

But, on the other hand, the intensity of the thermo-current is also proportional to the difference between the temperatures of the middle of the bar and the points A (fig. 5), the latter of which have the temperature of the surrounding air. Hence, if we denote by W the resistance of the whole circuit, by V_m and U (as before) the temperature of the middle of the bar and the surrounding air, then

$$i = \frac{\alpha(\mathrm{V}_m - \mathrm{U})}{\mathrm{W}}, \quad \cdots \quad (9)$$

where α denotes the electromotive force which corresponds to a temperature-difference of $1°$ C. From (8) and (9), therefore, if

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* The quantity $f(0)$ is the torsion-moment exerted upon the needle in the plane of the meridian by the multiplier when it is traversed by the unit of current. In the previously mentioned memoir (Pogg. *Ann.* vol. cxxxvii. p. 134) it is denoted by D. There it was found that

$$f(0) = 2\cdot8884\,.\,\frac{m}{\sqrt{\mathrm{L}}}\,\sqrt{\frac{\mathrm{W}}{k}},$$

in which L denotes the length of the needle, k the specific resistance, and W the total resistance of the galvanometer-wire. For the above-mentioned galvanometer it was found from direct observations that $f(0) = 48\cdot09\,.\,m$; while, according to the formula given, $f(0) = 47\cdot41\,.\,m$ if $k = 219274\dfrac{\text{millimetres}}{\text{second}}$. Probably the value of k on which the calculation is based is somewhat too great.

for shortness we put

$$R = \frac{WTm}{af'(0)}, \quad \ldots \ldots \ldots (10)$$

we obtain

$$V_m - U = R\frac{\tan\phi}{1 + a\sin\phi^2}. \quad \ldots \ldots (11)$$

Accordingly, when the quantities R and a are known, and ϕ and U observed, the temperature of the middle is thereby given. The quantity a had been determined after the method given by Poggendorff*. As the above galvanometer (constructed by myself) had perfectly regular coils, and care had been taken to secure a good centring of the multiplier, the values of a, calculated from various angles of torsion of the multiplier, exhibited a satisfactory accordance. For example, a set of such observations gave for a:—

$$-0\cdot6328$$
$$-0\cdot6259$$
$$-0\cdot6265$$
$$-0\cdot6316$$
$$-0\cdot6274$$

Mean . . $-0\cdot6288$

A second set gave similar results. For the following observations the value

$$a = -0\cdot63$$

is perfectly sufficient. The quantity R depends on the resistance W of the circuit, and therewith upon the temperature of the galvanometer-wire. An alteration of this temperature could have been easily reckoned according to the known law of the variation of the resistance with the temperature; but as the temperature of the wire cannot be accurately determined, it was preferred to determine R specially before each series of observations.

At the beginning of each series of experiments, before the ends of the bar were exposed to a heating or cooling, the temperature of the bar was that of the surrounding air. To control this, the conduction to the middle was inserted, so that the needle showed no deflection. A vessel with heated oil was now approached from beneath to the points and the thermometer between them (fig. 5) until they were in the centre of the oil. The vessel containing the oil was suspended free in a cylindrical vessel of wood of 1 inch thickness (fig. 6), upon which was a wooden lid with a circular aperture. Through this the points and thermo-

* Pogg. *Ann.* vol. lvii. p. 324.

meter were introduced into the oil, and then the aperture was closed with cotton wool. In this way an extreme retardation of the cooling of the oil was attained (in 45 minutes it amounted, on the average, to about 5° C.), and the temperature of oil, points, and thermometer could at any moment be taken as equal. A second thermometer was suspended by the side of the bar, giving the temperature of the surrounding air, and consequently that of the bar. During the experiments the temperature of the air remained nearly constant. The two thermometers were read off with the aid of a telescope. It is now easy, from simultaneous observation of the temperatures of the oil and the bar, and of the accompanying deflection of the needle, to deduce, according to equation (11), the value of R. For this a single simultaneous observation would indeed have sufficed; yet, for control, several such observations were always made. The determination of R in the first series of experiments with the iron rod may serve as an example.

Deflection of the needle.	Temperature of the oil.	Temperature of the middle of the rod.	Difference.	R.	Calculated difference.
5 19·3	29 16′ C.	4·85 C.	21·31	259·5	24·32 C.
4 22·2	24 70 „	4·80 „	19·90	259·4	19·92 „
3 24·5	20 32 „	4·78 „	15·54	260·3	15·50 „

From this we obtain the mean value

$$R = 259·7.$$

$$V_m - U = 259·7 \frac{\tan \phi}{1 - 0·63 \sin \phi^2}. \quad \cdots \quad (12)$$

The numbers in the sixth column are obtained by calculating the differences of temperature from the observed angles ϕ. In those experiments in which the ends of the iron rod were alternately heated and cooled, the points A (fig. 5) possessed the lower, the middle of the rod the higher temperature, and, accordingly, the current was in the opposite direction to that in the above experiments. With the aid of the regulator, however, it was easy to make the deflection of the needle follow the same direction. Besides, no particular stress need be laid upon the deflection being in the same direction, since before the experiments the turns of the multiplier-coil were placed parallel to the plane of the magnetic meridian, and the deflections, when a constant current passed through the multiplier in either direction, were equally great on both sides of the position of rest.

In order to determine the quantity p in accordance with equations (6), when the rod had assumed the state of periodic limits the conduction to H and I was connected with the galvanometer. A motion of the needle then commenced, variable with the time θ. The forces on which this motion depends are (1) the horizontal component of the earth's magnetism, (2) the deadening force, (3) the force exerted by the thermo-current upon the needle. The torsion-moment which the first two forces exert upon the needle with a deflection ϕ are

$$-\mathrm{T}m \sin \phi, \quad -\frac{[f(\phi)]^2}{w}\frac{d\phi}{dt},$$

T being the horizontal component of the earth's magnetism, m the magnetic moment of the needle, and w the resistance of the circuit. In order to determine also the torsion-moment of the third force, we must consider that the intensity of the thermo-current is a quantity which varies with the time, whereby an induction of the current upon itself is occasioned, and the consequence is a diminution of the intensity. For, let i be the intensity of the thermo-current which is actually passing through the circuit at the time θ, i_1 the intensity which would be observed if the current remained constant, then is

$$i = i_1 - \gamma\frac{di_1}{d\theta} + \gamma^2\frac{d^2 i_1}{d\theta^2} - \gamma^3\frac{d^3 i_1}{d\theta^3} + \ldots,$$

where γ is a constant depending on the resistance and the form of the circuit. According to equations (6), however, in an even period

$$i_1 = \frac{\alpha(\mathrm{M} - \mathrm{N}e^{-p\theta})}{w},$$

in an odd period

$$i_1 = \frac{\alpha(-\mathrm{M}' + \mathrm{N}e^{-p\theta})}{w}.$$

From this it follows that the intensity i actually present in the circuit at the time θ can be represented by an expression of the form

$$i = \frac{\alpha(\mathrm{A} + \mathrm{B}e^{-p\theta})}{w},$$

where A and B are constants. Hence the torsion-moment of the third force becomes

$$+\frac{\alpha(\mathrm{A} + \mathrm{B}e^{-p\theta})}{w}f(\phi),$$

and we obtain for the equation of motion of the needle of the galvanometer, if κ denotes the moment of inertia of the needle

and the suspension :—

$$\frac{d^2\phi}{d\theta^2} + \frac{[f(\phi)]^2}{\kappa w}\frac{d\phi}{d\theta} + \frac{\mathrm{T}m}{\kappa}\sin\phi - \frac{\alpha(\mathrm{A}+\mathrm{B}e^{-p\theta})}{\kappa w}f(\phi) = 0.$$

It is only for small angles ϕ that the integration of this equation can be effected in a definitive form. Presupposing small angles, it is transformed into

$$\frac{d^2\phi}{d\theta^2} + \frac{[f(0)]^2}{\kappa w}\frac{d\phi}{d\theta} + \frac{\mathrm{T}m}{\kappa}\phi - \frac{\alpha(\mathrm{A}+\mathrm{B}e^{-p\theta})}{\kappa w}f(0) = 0,$$

from which by integration we obtain

$$\phi = \frac{\alpha f(0)}{w\mathrm{T}m}\mathrm{A} + \frac{\alpha f(0)}{\kappa w}\frac{\mathrm{B}e^{-p\theta}}{p^2 - \frac{[f(0)]^2}{w\kappa}p + \frac{\mathrm{T}m}{\kappa}}$$
$$+ \mathrm{C}e^{-\frac{1}{2}\frac{[f(0)]^2}{m\kappa}\theta}\sin\left\{(\mathrm{C}'-\theta)\sqrt{\frac{\mathrm{T}m}{\kappa} - \frac{1}{4}\left(\frac{[f(0)]^2}{w\kappa}\right)^2}\right\}.$$

C and C′ here denote two integration constants. From this we perceive that the motion of the needle is composed of two motions superposed the one upon the other—first, of that which the needle would have if no thermo-current were present, and then of that which it would have if the periodic part were omitted.

If now we construct a galvanometer with great deadening force, so that the needle approximates to the aperiodic state, as was done in the present case, and make the time of an oscillation t of the needle and the resistance external to the galvanometer small, then the logarithmic decrement $\lambda = \frac{1}{2}\frac{[f(0)]^2}{w\kappa}$, t will have a great value, and the periodic part in the motion of the needle will after the lapse of a very short time be vanishingly small. Consequently, if at the commencement of each period we let a certain time elapse before we begin the observation of the deflections ϕ corresponding to the different times θ—which, besides, is necessary in order that we may employ the simplified expressions of $\mathrm{D}_{2\mu}$ and $\mathrm{D}_{2\mu+1}$, equations (6),—the periodic part may be omitted, and we obtain for ϕ simply an expression of the form

$$p = \mathrm{P}' + \mathrm{Q}'e^{-p\theta},$$

where P′ and Q′ are constants. If these observations are made with mirror and scale, it is more convenient to introduce the arcs of double the angles of deflection instead of the deflections ϕ themselves. Let r be the distance of the mirror from the scale, s the arc corresponding to the angle ϕ, then $s = 2r\phi$, and we have

$$s = \mathrm{P} + \mathrm{Q}e^{-p\theta}, \quad . \quad . \quad . \quad . \quad . \quad (13)$$

—where P and Q denote the same constants as before, multiplied by 2r. The angles s were not, indeed, observed directly on the scale, but their tangents; these, however, can easily be referred to the corresponding arcs by a well-known reduction.

Let σ denote the observed number of scale-divisions, corresponding to the angle ϕ, and let r be expressed in parts of the scale, then the corresponding arc

$$s = \sigma - \frac{\sigma^3}{3r^2}.$$

In the same way, from a series of observed values of σ corresponding to certain times θ, p can be found.

For the logarithmic decrement when the conduction to the points H and I of the rod was connected with the galvanometer, there was obtained

$$\lambda = 3 \cdot 704$$

with a time of an oscillation

$$t = 6'' \cdot 904,$$

whence it follows that at the end of 15 seconds the periodic part of the motion of the needle had already vanished, even when the deflections were the greatest. The sensitiveness, however, of the galvanometer was such that the deflections were too great. This inconvenience can be remedied in two ways—either by increasing the resistance of the circuit, or by strengthening the horizontal component of the earth's magnetism by placing a suitable magnet in the meridian-plane. But in the former case the deadening is diminished in equal proportion, which is unfavourable for the observations; in the latter case the deadening remains unaltered, and the sensitiveness can be regulated as we choose. The latter, therefore, was the procedure adopted in the following observations.

Observations.

After the preliminary observations for the determination of the quantity R, the ends of the rod to be investigated were, at equal intervals of time T, alternately heated and cooled. At the end of about three quarters of an hour the extreme periodic condition commenced, being recognized by the temperature of the middle of the rod no longer increasing. Before the first series of observations, and between each two of the succeeding ones, the temperature U of the vicinity, the temperature u_0 of the water, the position of rest of the needle, and the scale-division corresponding to the temperature V_m of the middle of the rod were read off. The temperature of the steam was ascertained from the state of the barometer. It now appeared that the dif-

ference between the temperature of the middle of the rod and that of the surrounding air was not perfectly constant, as it should have been according to the theory, but that variations occurred which might have for their consequence deviations of half a degree from the mean value. It may be that the cause of this is to be sought in the non-accordance of the bases of the theory with what takes place in reality, or in the place where the middle wires were soldered to the rod not coinciding exactly with its centre. But in this case there would necessarily have been a certain regularity in the variations, which, however, was not observed. It is therefore probable that they were occasioned by air-currents, which, notwithstanding all precautions, could not be entirely avoided.

Each series of observations consisted of an even number of readings $s_0, s_1, \ldots s_{2n-1}$, which were made in regular intervals of time $\delta = 15''$, the first of which fell on $\theta_0 = 45''$. In order from these observations to calculate the value of p according to equation (13), it appears the simplest to form the differences $(s_0 - s_1)$, $(s_1 - s_2)$, $(s_2 - s_3)$, \ldots and to divide these by one another. A little consideration, however, shows that herein the unavoidable errors of observation must have a very important influence on the result. The following treatment leads to more accurate results.

If we first form the differences

$$(s_0 - s_n), \ (s_1 - s_{n+1}), \ldots (s_{n-1} - s_{2n-1}),$$

we obtain a series of equations of the form

$$s_0 - s_n = Q(e^{-p\theta_0} - e^{-p\theta_n}),$$
$$s_1 - s_{n+1} = Q(e^{-p\theta_0} - e^{-p\theta_n})e^{-p\delta},$$
$$s_2 - s_{n+2} = Q(e^{-p\theta_0} - e^{-p\theta_n})e^{-2p\delta},$$
$$\cdot \qquad \cdot \qquad \cdot \qquad \cdot \qquad \cdot$$

If we take the logarithms on both sides and denote $\log(s_0 - s_n)$, $\log(s_1 - s_{n+1})$, \ldots by $m_0, m_1, \ldots$ and for abbreviation put

$$\log Q(e^{-\theta_0} - e^{-p\theta_n}) = x, \quad \log e^{-p\delta} = y,$$

there result equations of the form

$$m_0 = x,$$
$$m_1 = x + y,$$
$$m_2 = x + 2y,$$
$$\cdot \quad \cdot \quad \cdot \quad \cdot,$$

from which, by the method of least squares, x and y, and with them p and Q, were found. With the aid of these values we

can, for control, calculate the constant P corresponding to each *s*, which must come out equal for each of the values.

All the following observations were calculated in this way. The fundamental units of measurement employed are *millimetre*, *milligramme*, and *second*; and accordingly the heat-unit is that amount of heat which is capable of raising the temperature of 1 milligramme of water from 0° C. to 1° C.

I. *Iron rod (annealed).*

Length . . $l = 230 \cdot 35$ millims. $D = 7 \cdot 761$ $T = 5'$
Diameter . . $=$ $7 \cdot 5168$ millims. $C = 0 \cdot 1125$
Distance of the mirror from the scale $r = 2927$ millims.
      $= 2952 \cdot 4$ parts of the scale.

The density and the specific heat were specially determined, the latter according to the method proposed by Neumann. The temperature-formula has been already given (equation 12).

An even period.

θ.	σ.	$\dfrac{\sigma^3}{3r^2}$.	*s*.	s calculated.	Difference.	P.
0′ 45″	−440	3·2	−436·8	−441·5	+4·7	729·8
1 0	−215	0·4	−214·6	−213·9	−0·7	724·4
15	− 32	0·0	− 32·0	− 30·6	−1·4	723·7
30	+117	0·1	+116·9	+116·9	0·0	725·1
45	+235	0·5	+234·5	+235·5	−1·0	724·1
2 0	+331	1·4	+329·6	+331·1	−1·5	723·6
15	+410	2·6	+407·4	+408·0	−0·6	724·5
30	+475	4·1	+470·9	+469·8	+1·1	726·2
45	+526	5·6	+520·4	+519·7	+0·7	725·8
3 0	+567	7·0	+560·0	+559·7	+0·3	725·4
15	+601	8·3	+592·7	+592·0	+0·7	725·8
30	+627	9·4	+617·6	+618·0	−0·4	724·7
45	+648	10·4	+637·6	+638·9	−1·3	723·8
4 0	+667	11·3	+655·7	+655·7	0·0	725·1
15	+681	12·1	+668·9	+669·2	−0·3	724·8
30	+693	12·7	+680·3	+680·1	+0·2	725·3
					Mean...	725·1

Further, the height of the barometer, reduced to 0° C., was 758·97 millims.

$$u_1 = 99 \overset{\circ}{\cdot} 96 \text{ C.} \qquad U = \overset{\circ}{5} \cdot 37 \text{ C.}$$
$$u_0 = 4 \cdot 72 \text{ C.} \qquad V_m = 33 \cdot 14 \text{ C.}$$

$K = 15 \cdot 07$ heat-units. $H = 0 \cdot 00267$ heat-unit. In like manner in the following periods we obtained the values :—

Even periods.

$u_1 = 99\cdot96$	$U = 5\cdot47$	$K = 15\cdot09$
$u_0 = 4\cdot71$	$V_m = 33\cdot22$	$H = 0\cdot00266$
$u_1 = 99\cdot96$	$U = 5\cdot54$	$K = 15\cdot29$
$u_0 = 4\cdot71$	$V_m = 33\cdot16$	$H = 0\cdot00271$
$u_1 = 99\cdot96$	$U = 5\cdot59$	$K = 15\cdot10$
$u_0 = 4\cdot52$	$V_m = 33\cdot32$	$H = 0\cdot00264$
$u_1 = 99\cdot96$	$U = 5\cdot69$	$K = 15\cdot16$
$u_0 = 4\cdot52$	$V_m = 33\cdot49$	$H = 0\cdot00262$

Odd periods.

$u_1 = 99\cdot96$	$U = 5\cdot97$	$K = 14\cdot39$
$u_0 = 4\cdot52$	$V_m = 33\cdot03$	$H = 0\cdot00261$
$u_1 = 99\cdot96$	$U = 6\cdot00$	$K = 14\cdot40$
$u_0 = 4\cdot52$	$V_m = 33\cdot01$	$H = 0\cdot00261$
$u_1 = 99\cdot96$	$U = 6\cdot04$	$K = 14\cdot68$
$u_0 = 4\cdot54$	$V_m = 32\cdot99$	$H = 0\cdot00267$
$u_1 = 99\cdot96$	$U = 6\cdot10$	$K = 14\cdot76$
$u_0 = 4\cdot62$	$V_m = 32\cdot86$	$H = 0\cdot00273$

If we take the mean of the values of K and H given by the even periods, and likewise of those derived from the odd periods, we obtain:—

$$K = 15\cdot14, \qquad H = 0\cdot00266;$$
$$K = 14\cdot56, \qquad H = 0\cdot00266.$$

The difference which appears here between the values resulting from the even periods and those resulting from the odd ones is to be accounted for by a want of absolute coincidence of the soldering-places with the points $x = \frac{1}{6}l$ and $x = \frac{4}{6}l$; for a small deviation of the first-mentioned place may have a considerable influence. The mean of the whole is:—

$$K = 14\cdot85, \qquad H = 0\cdot00266.$$

As both K and H are variable with the temperature, we must, in order as far as possible to apply calculation to the variability, refer these values to the mean temperature of the rod. Denoting this by Δ, we find

$$\Delta = \frac{1}{2l\mathrm{T}} \int_0^t dx \int_0^\mathrm{T} (V_{2\mu} + V_{2\mu+1}) d\theta.$$

With the aid of equations (2), if for abbreviation we put

$$a = e^{\frac{l}{2}\sqrt{\frac{\overline{HP}}{KQ}}}, \quad b = e^{-\frac{b}{2}\sqrt{\frac{\overline{HP}}{KQ}}},$$

we accordingly obtain

$$\Delta = U + \frac{ue + u_1 - 2U}{l\sqrt{\dfrac{\overline{HP}}{KQ}}} \cdot \frac{a-b}{a+b}. \quad \cdots \quad (14)$$

If we introduce into this expression the mean values of u_0, u_1, U, H, K, the result is

$$\Delta = 39°{\cdot}23 \text{ C.}$$

Consequently, if one surface of an infinitely large iron plate of *one millimetre* thickness be kept at the temperature of 39° C., and the other at 38° C., through *one square millimetre* area 14·85 *heat-units* will pass in a *second* from one surface to the other[*]. If, further, a body consists of iron the surface of which has the same constitution as our iron rod, and if this body be constantly kept at the temperature of 39° C. while the temperature of the surrounding air is 38° C., from *every square millimetre* surface 0·00266 *thermal unit* will be given up to the air in *one second*.

The surface of the iron rod had not the highest polish. In order to try whether the quality of the surface exerted a perceptible influence on the value of K, the previous experiments were repeated with the same iron rod after its surface had been evenly coated with soot from a gas-flame. The thickness of the coating was such that the metallic surface was just perceptible through it. Here the duration of the periods amounted to 10 minutes, in order to make the observations in even and odd periods alternately, not consecutively as before.

II. *Iron rod coated with soot.*

Even periods.

$$K = 14{\cdot}80 \qquad H = 0{\cdot}00333$$
$$K = 15{\cdot}21 \qquad H = 0{\cdot}00337$$

Odd periods.

$$K = 14{\cdot}65 \qquad H = 0{\cdot}00319$$
$$K = 14{\cdot}50 \qquad H = 0{\cdot}00322$$

Mean values.

$$K = 14{\cdot}79 \qquad H = 0{\cdot}00328$$

We consequently obtain approximately the same value for K, which seems to indicate that, in this method of determining the heat-conducting power, the heat given up to the environs does

[*] By multiplying the above by 0·6 we obtain the thermal conducting-power in the units used by Ångström.

not exercise any prominent influence. That, notwithstanding the great exposing-power of soot, the quantity H does not assume a greater value is to be attributed to the bad conductivity of soot for heat.

III. *Drawn German-silver rod (annealed).*

Length . . $l = 230 \cdot 4$ $C = 0 \cdot 0944$
Diameter . . $= 7 \cdot 622$ millims. $D = 8 \cdot 621$

C and D were, as before, determined specially.

Odd periods.

$$K = 8 \cdot 404 \qquad H = 0 \cdot 00318$$
$$K = 8 \cdot 405 \qquad H = 0 \cdot 00316$$

Even periods.

$$K = 7 \cdot 832 \qquad H = 0 \cdot 00291$$
$$K = 7 \cdot 791 \qquad H = 0 \cdot 00291$$
$$K = 7 \cdot 817 \qquad H = 0 \cdot 00295$$

Mean value from the even periods.

$$K = 7 \cdot 813 \qquad H = 0 \cdot 00292$$

Mean value from the odd periods.

$$K = 8 \cdot 404 \qquad H = 0 \cdot 00317$$

Mean of the whole.

$$K = 8 \cdot 108 \qquad H = 0 \cdot 00304$$

These latter values refer to the mean temperature Δ of the German-silver rod,

$$\Delta = 31° \cdot 25 \text{ C.}$$

The determination of the mean temperature to which the values of K and H were referred can only be regarded as a rough approximation. A step would be taken towards greater accuracy if, in constructing the general equations for the propagation of heat, K and H were introduced as functions of the temperature, putting $K = K_0(1 + \alpha t)$, $H = H_0(1 + \beta t)$. In fact, presupposing this, the *stationary* condition in a rod whose ends are kept at constant temperatures can be determined generally[*]. The solution consists in an integration which can be effected in a definitive form for a rod unlimited in one direction. Great difficulties, however, oppose an experimental use of this solution for the determination of α, β, K_0, and H_0.

A knowledge of the dependence of K and H on the temperature would also be desirable, in order to test the conjecture of

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[*] Poisson has given an approximate solution of this problem (*Théorie mathématique de la Chaleur*, p. 255).

Wiedemann and Franz[*], that the heat-conducting power of metals is directly proportional to their electric conductivity, or inversely as their specific resistance. If w denotes the specific resistance of a metal, C a constant, the heat-conducting power

$$K = \frac{C}{w}.$$

Lenz[†] has found this law verified when K and w refer to the same temperature. In a determination of K in the thermal units above chosen, and of w in absolute measure $\frac{\text{millimetres}}{\text{second}}$, for one and the same iron rod, I obtained for the constant C

$$C = 2458 \times 10^4,$$

in which the values of K and w referred to the temperature 44°·3 C.

LX. *Supplementary Remarks on some early Logarithmic Tables. By* J. W. L. GLAISHER, *B.A., F.R.A.S., Fellow of Trinity College, Cambridge* ‡.

IN the October Number of the Philosophical Magazine I stated that Decker's work left no doubt "that to him must be assigned the credit of having been the first foreigner who published Briggian logarithms, an honour which has always been hitherto assigned to Vlacq." This sentence requires some modification, or at all events explanation, as Vlacq was not the only claimant for the honour in question. His rival was Denis Henrion, who published at Paris in 1626 a *Traicté des Logarithmes*, containing Briggs's logarithms of numbers from 1 to 20,000 to ten decimals, and Gunter's logarithmic sines and tangents. Henrion's work has been so rarely met with by the bibliographers that it has become little more than a tradition. It is scarcely ever mentioned by German writers; and all De Morgan could collect is contained in the following extract:—" (1626) Henrion's 'Logarithms,' Paris. (Dodson, followed by Hutton.) Lalande knew nothing of this work, nor Delambre. All we can learn is from Dechales, who states that Henrion wrote on the proportional compasses in (1623), reprinted in (1681), and on the rule of proportion (which we take to be Gunter's scale) in (1626); and that the last work contains logarithms of numbers up to 2000."

There is, however, a copy in the British Museum, acquired in 1854; and as it bears the name of H. C. Schumacher, Copenhagen, 1816, it no doubt belonged to the celebrated astronomer. The preface is not dated; and there is no "privilége" or date* of completion of printing (or perhaps the last page is torn out) ; so that there exists no means of deciding whether the priority of publication belongs to Decker or Henrion. It struck me as not unlikely that there really was perhaps no "privilége," as the "privilége" to Henrion's *Cosmographie*, 2nd edit., Paris, 1626, includes "toutes ses œuvres," and is dated Sept. 7, 1624, the "achevé d'imprimer" being April 1626; but the *L'Usage du Mecromètre*, 1630, by the same author, has the "privilége" as usual. In the preface to his *Traicté des Logarithmes* Henrion makes no reference to Wingate's publication at Paris the previous year; he states that he had calculated some logarithms himself when the appearance of Briggs's *Arithmetica* rendered the further progress of his work unnecessary. Lalande mentions five books of Henrion; and I have met with three or four others; so that he must have been a somewhat prolific mathematical writer for the time : he describes himself on his titlepages as "demeurant en l'isle du Palais."

Of Decker's *Eerste Deel vande Nieuwe Telkonst* I have found another copy in the library of Trinity College, Cambridge, and have therefore been enabled to examine it at leisure. In the preface Decker remarks that, when he was professor of geometry and arithmetic, he noticed the great fear and dislike evinced towards the arts that involved long calculations, and, being very anxious to find a remedy for this, he carefully studied every new mathematical work on its appearance, and amongst others Napier's *Canonis Logarithmorum Mirifici Descriptio*. This book (which Vlacq translated for him, as he was ignorant of Latin) pleased him exceedingly; but he saw that it was unsuitable for the general public. Soon after, Vlacq called his attention to and translated for him Napier's *Rabdologia*†, which completely realized his desires. The announcement contained in it about the other method of logarithms (viz. the adoption of the decimal base) led him to procure from England Briggs's *Arithmetica* and Gunter's Table of sines and tangents. These pleased him so much and

* Even if a copy with a "privilége" were produced, it would not afford a very satisfactory decision. Decker and Henrion may very well divide the honour.

† Nothing shows better the fear with which arithmetical calculations were regarded than the eagerness with which the *Rabdologia* was welcomed. I have seen an Italian translation by Locatello, Verona, 1623, and an edition by Ursinus, 1623 (not 1723 as given by Rogg), as well as references to two or three others (in booksellers' lists), besides subsequent English editions.

were so unknown in Holland, that he determined to publish them all in Dutch, " incited thereto by the accomplished Adriaen Vlack, with the promise that he would not spare help nor labour till the completion of the work." Decker then explains the arrangement of his work, viz. that it is to consist of two parts, of which the first (that under notice) contains the *Rabdologia* and some developments of his own on commercial arithmetic; he then gives a few details about the translation, and some advice about the construction of the instruments required in the *Rabdologia*. After this follows a semi-apology for the use of Greek letters in the diagram of the Board in the *Local Arithmetic*, which he extenuates by pointing out that any other marks would do as well; and the preface concludes with an exhortation to the reader to wait with patience till the *Tweede Deel* is ready. Decker's own interest-tables are decimal, and Stevinus's tract *La Disme* is appended in Dutch (*De Thiende* *). Apart from the mention of Vlacq and the interest attaching to Decker as one of the first who appreciated the invention of logarithms, his book is of value in reference to the spread of decimal arithmetic. On the whole he seems to have been an intelligent and useful worker, and one who merited more than he has received.

I may mention that I have examined all the books published on logarithms during the twenty years following the first announcement by Napier in 1614 that are to be found in the libraries in the British Museum, the Royal Society, the Cambridge University, the Greenwich Observatory, and other institutions, and have thus been enabled to form *from inspection* a nearly complete bibliography of the works on the subject that appeared during this period. There are only five or six books or editions that I have not yet succeeded in seeing; so that the formation of a perfect bibliography for the time in question will probably be not nearly so difficult a matter as one would *à priori* suppose. The Oxford libraries ought to be rich in books on the subject†, as Briggs was Professor there, and the Bodleian possesses most of his manuscripts. There are also several valuable mathematical libraries at present under arrangement, which will be probably accessible for research within a reasonable time; so that the mathematical historian or bibliographer in England will soon be placed in a much improved position with regard to sources of information.

* Whether this famous tract first appeared in French or Dutch I do not know. De Morgan speaks of a French edition, *La Disme*, 1585; and I have a Dutch edition of the same date (*De Thiende*, Leyden, 1585) before me as I write.

† On referring to the Catalogue of the Bodleian Library (1843), I find the collection of early logarithmic Tables there is by no means remarkably good.

I have therefore hopes of being able to make my list nearly complete. A good deal of research and trouble has been devoted to the subject by the German bibliographers; but, for want of the direct evidence derived from inspection of the books (the majority being English, and no doubt even less common in Germany than here), they have been frequently led into error by the inaccurate descriptions given in the second-hand sources whence they have been obliged to derive their information. There is no account which is not very inaccurate in its details. I will only refer here to one error that is universal. Hutton says that on Briggs's return to London in 1617 "he printed the first thousand logarithms to eight places of figures, besides the index, under the title of *Logarithmorum Chilias Prima.*" This is the first publication of Briggian logarithms, and is therefore of historical importance; but no one seems to have seen the book itself; and Hutton's statement has been quoted by every subsequent writer without verification. There is, however, a copy in the British Museum; and the Table contains the logarithms of the first thousand numbers to *fourteen* decimals. The whole forms a small octavo tract of sixteen pages, and has neither author's name, place, nor date*. There is a brief preface, containing the often quoted remark that *it is to be hoped* that Napier's posthumous work will explain why the logarithms are different from those in the *Canon Mirificus*; and it concludes with the motto "In tenui, sed non tenuis fructusve laborve." Dodson does not mention this tract. Ward, in his ' Lives of the Professors of Gresham College,' 1740, incorrectly says that it was printed in 1617, *as appears by the titlepage.* Hutton's account is inaccurate as noted above; and De Morgan never saw the book and gives no account, while the foreign bibliographers rarely mention it. There has also been some difficulty about the date; the facts are :—that Sir Henry Bourchier wrote on Dec. 6, 1617, to Dr. Usher†, "Our kind friend, Mr. Briggs, hath lately published a supplement to the most excellent Tables of logarithms, which I presume he has sent to you;" but as Napier's posthumous works are spoken of in the preface, while his death did not take place till April 3, 1618, it has been supposed that some incomplete copies may have been circulated in 1617, but that the real date of publication was 1618. Ward notices the difficulty; and Hutton regarded 1618 as the true date; Rogg also assigns 1618. Nearly all the writers before Hutton either knew nothing about the book, or confounded it with

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* No doubt one reason why the tract has been seen by so few is that in the Museum Catalogue, instead of being entered under "Briggs," 1617, it appears only under "Logarithms," with the date "[1695?]."

† Quoted by Ward, ' Lives of the Gresham Professors,' p. 122.

Wright's English translation of the *Canon Mirificus* in 1616. The statement, however, given on the authority of Mr. Mark Napier (in his 'Memoirs of John Napier of Merchiston,' 1834), that Napier died on April 4, 1617, explains the matter, and shows that the publication did take place in 1617. The other facts also agree : in 1614 Napier published the *Canon Mirificus* ; Briggs visited him at Merchiston in 1615 and 1616, and intended to pay a third visit in the summer of 1617 to show him his work. Dodson says that Briggs's logarithms were published with Gunter's *Canon Triangulorum* in 1620 ; and in the only copy of Briggs (or Gunter) that I have been able to see, the two are bound up together. It is not unlikely that Gunter did issue Briggs's *Chilias* with his *Canon* ; and if so, the copies were most likely originally printed in 1617, and were not reprints. Henrion mentions that he received Briggs's *Chilias* with Gunter's *Canon* ; but as both the Latin and English editions of the latter appear in the Bodleian Catalogue, it is not worth while discussing a question so easy to set at rest.

With reference to the relations between Napier and Briggs, with regard to the invention of decimal logarithms, it seems, after reading the facts, hard to believe that they could have formed matter for controversy. The statements of Napier and Briggs both agree in all particulars ; and the warmest friendship subsisted between them. Napier at his death left his manuscripts to Briggs ; and all the writings of the latter show the greatest reverence for him. Hutton, though stating the facts correctly, has unfortunately imputed to Napier want of candour, a charge which the evidence he adduces in no way justifies. Mr. Mark Napier, in his " Memoirs," referred to above, has successfully refuted this imputation, but he has fallen into the opposite extreme of extravagantly eulogizing Napier and depreciating Briggs ; he attributes Hutton's assertions to national jealousy ! Mr. Napier's book, though published nearly forty years ago, has not been much referred to ; and it is scarcely to be expected that many will care to pick out from a quarto volume of 534 pages of diffuse writing the slight additional matter it contains. Hutton's history of logarithms is generally accurate and truthful ; and it is a matter of regret that he should have systematically interpreted Briggs's remarks in a manner clearly contrary to their true meaning, and the more so as there unquestionably existed between the inventor of logarithms and his friend an attachment almost unique in science. Hutton's account has now remained the standard work of reference for nearly a century, and his views have been adopted in a more or less modified form by Delambre and Montucla ; so that it will be long before the simple

facts will become generally known apart from Hutton's gloss*. One copy of the *Canon Mirificus* of 1614 that I have seen, viz. that in the Greenwich Observatory Library, is without the final *Admonitio*, in which Napier apologizes for any errors that may have crept into the Tables on the ground of his health and the work having been done all by himself, &c.; and there is no possibility of its having been torn out, as in other copies it is printed on the back of the last page of the Tables†. Mr. Mark Napier mentions in a note that he has seen such a copy; but in the text he assumes Briggs to have had one with the *Admonitio*. There are signs that Hutton had not seen this *Admonitio*. I have collected together the few statements in Napier, Briggs, &c. that bear upon the invention of decimal logarithms, but refrain from publishing them till I have completed the bibliography of the period, so as incidentally to reproduce as few errors as possible; but the most important quotations on the matter are to be found in Hutton.

It is scarcely necessary to mention that in the note on p. 296 of

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* Since this was written, Mr. Sang, of Edinburgh, has circulated some specimen pages of his proposed nine-figure logarithmic Table, in which he states that "John Nepair, the illustrious inventor of logarithms, having computed trigonometrical Tables according to that particular system which bears his name, perceived and announced the far greater advantages to be derived from the Denary system. He carefully explained the process to be followed, and delegated the actual calculation to his friend Henry Briggs, of the University of Oxford." This certainly conveys an incorrect impression. Briggs's own words on the title-page of the *Arithmetica*, 1624 (I quote the English translation of 1631), are:—"These numbers were first invented by the most excellent John Neper, Baron of Marchiston; and the same were transformed and the foundation and use of them illustrated with his approbation [ex ejusdem sententia] by Henry Briggs." Elsewhere Briggs states that when he suggested the advantage of decimal logarithms to Napier, the latter told him he had already thought of them, and pointed out a slight improvement (viz. that the characteristics of numbers greater than unity should be positive, instead of negative as Briggs proposed). Briggs was a distinguished mathematician and not a mere computor. Decimal logarithms doubtless occurred to Napier and Briggs independently; but it was the latter who developed the idea and formed the Tables; and that he would have done so even if he had never visited or corresponded with Napier, there is good reason to believe. The statement that Napier "carefully explained the process to be followed," is supported by no evidence. That Napier did give Briggs assistance is likely enough; and the probability is increased by the fact that Briggs's method of calculation differs very little from that explained in Napier's *Constructio*; but such assistance must have been given privately, if at all, as Briggs's *Chilias* appeared in 1617, and Napier's *Constructio* not till 1619; Briggs also was quite mathematician enough to have been able to investigate the method of computing logarithms for himself after reading the *Descriptio*. I may mention that Briggs, though Professor at Oxford towards the close of his life, was educated at Cambridge.

† Of four other copies of the *Canon Mirificus* of 1614 that I have seen, three have the *Admonitio*, and the other has the last page torn out.

my former paper, 1820 is a misprint for 1620. With reference to the remark of Norwood about Vlacq's work, quoted in the note on p. 301, I may add that I have since seen the first edition, 1631, and the third, 1656, in both of which the passage in question occurs in the " advertisement to the Reader." It has occurred to me that Norwood's remark was intended to apply rather to the prefixed Trigonometry &c. than to the Tables; and if this is the case, there is some justification for it.

Trinity College, Cambridge.
    October 17, 1872.

LXI. *Researches on Elective Attraction.* By Edmund J. Mills, *D.Sc.*[*]

> " Jamne vides igitur magni primordia rerum
>   Referre, in quali sint ordine quæque locata,
>   Et commista quibus dent motus accipiantque."
>   *Lucretius.*

THE following experiments had their origin in an attempt to prepare nitrylic chloride by the action of phosphoric oxychloride on plumbic nitrate. The reaction between these two bodies takes place, according to the common statement[†], in accordance with the following symbolic expression :—

$$3\,Pb\,(NO^3)^2 + 2\,POCl^3 = Pb^3\,(PO^4)^2 + 6\,NO^2\,Cl.$$
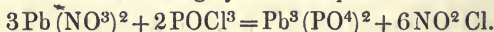
Among other modes of verifying this equation, the examination of the residue left behind when excess of the oxychloride is heated with the nitrate and then distilled off in a current of dry air was resorted to as the most simple and obvious. The results were found not to agree with the equation; and after three nitrates had been tried, a law of chemical attraction became evident, rendering the reaction worthy of pursuit for its own sake, although as a practical source of nitrylic chloride it had failed entirely. The nature and mode of establishment of this law constitute the subject of the present memoir.

When a nitrate is treated with phosphoric oxychloride as already described, the residue contains a chloride and a phosphate,—the latter being probably always phosphorylic phosphate $(PO \cdot PO^4)$ or phosphoric pentoxide. The ratio between these products is sensibly constant, and will be designated by a special symbol, in accordance with the following understanding :—

$$\alpha = \frac{\dfrac{\text{weight of chlorine}}{Cl}}{\dfrac{\text{weight of phosphoric oxide}}{P^2\,O^5}} = \frac{\text{weight of chlorine}}{\text{weight of phosphoric oxide}} \times 4.06.$$

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[*] Communicated by the Author.
[†] Compare Watts's ' Dictionary of Chemistry,' vol. iv. p. 77.

The nitrates to which this particular method of determining chemical activity applies must, as necessary conditions, be capable of perfect desiccation and actually amenable to the influence of the oxychloride. It was also advisable to exclude the nitrates derived from ammonia, amines, and amides, inasmuch as the employment of these would have rendered indispensable a preliminary inquiry into the phosphamides—bodies whose formulæ are for the most part at present unknown, but which would almost certainly occur, and have to be quantitatively determined in a mixture already sufficiently complex. Omitting baric nitrate, upon which, when it is dry, phosphoric oxychloride is without action, there appears, then, reason to believe that α can be satisfactorily valued in eight instances only.

Apparatus.—The preparation of a supply of air free from every trace of moisture is a well-known difficulty which has seldom been very satisfactorily overcome. For the purpose of these experiments an apparatus was eventually constructed which was found to answer every requirement. Seven glass vessels were filled to about one third of their capacity with pumice free from chlorine and supersaturated with oil of vitriol, and were connected with caoutchouc tubing both to each other and a gas-holder containing air. A wash-bottle containing oil of vitriol was added to these, to serve as a time-indicator; and the arrangement terminated with a long tube containing phosphoric followed by baric oxide. The volume of the air contained in this apparatus was at least four times as great as was required in any individual experiment. By closing it at both ends for twenty-four hours, the whole of the internal moisture would be removed; and on admitting a small stream from the gas-holder, it seemed highly probable that dry air only would leave the apparatus, even during the course of an entire operation.

The next portion consisted of a narrow inverted U-tube, which was followed by the "reaction-tube," wherein the chemical process was actually carried out. The latter, which had the shape of an ordinary "Liebig's drying-tube" (the body of which held about 7 cubic centims.), was used also for weighing the necessary materials, during which operation it was closed with a glass stopper and a caoutchouc fastening; in the process its body was immersed in an oil-of-vitriol bath, the heat of which could be regulated at pleasure. By means of a perforated cork, an inclined condensing-tube, about a foot long, was next attached. This was narrowed at the further extremity, so as to enter a small receiver, from which a tubulus, in its turn, carried the gaseous products of the reaction into a vessel containing lime.

Materials.—Details respecting individual nitrates will be alluded to more especially hereafter. However carefully pre-

pared, even with hydric nitrate distilled without ebullition, they were all found to contain iron, a circumstance which points to the existence of a ferric oxide volatile with the vapours of that nitrate. Hence it was necessary that they should all be fused, dissolved, filtered, and evaporated to dryness. When ordinary " pure nitric acid " was employed in their preparation, some sulphate was invariably found, doubtless owing to the known fact that hydric nitrate in large excess prevents the precipitation of baric sulphate, thereby rendering the impurity less easy of detection. The phosphoric oxychloride was prepared, according to Gerhardt's recommendation, by the action of hydric oxalate, dried at 90°, on phosphoric chloride. By carrying out the process in a large flask having a long neck closed with a watch-glass, and adding oxalate in slight excess, a highly satisfactory yield was produced. The oxychloride had next to be purified from hydric chloride (with which it was saturated) by three distillations ; on again distilling and collecting the last fourth of the distillate apart, a pure product was generally attained. This was secured in stoppered bottles holding each about 30 cubic centims., which were preserved under a desiccator containing lime and oil of vitriol ; when about 24 cubic centims. had been removed from any particular bottle, it was judged expedient to reject the remainder, on account of the accumulated error due to repeated contact with common air. A chlorine determination was made in each portion to be actually employed, the adoption or rejection of which was decided on the basis of the evidence thus procured. The product was colourless and did not fume in the air.

In the first experiments, not only the nitrate, but the oxychloride was weighed. It was soon evident, however, that the eye can easily form an adequate estimate of the sufficiency of such an excess of oxychloride as was here required ; and as, owing to the occasional commencement of action in the tube at the ordinary temperature, the weighing could not be generally performed with accuracy, it was afterwards dispensed with altogether. The amount of oxychloride taken was always more than enough to cover the nitrate.

The course of the actual performance of the reaction will be apparent from the details above given. The following experiments, however, to which all that succeed bear a substantial resemblance, are inserted as a proof of the efficiency of the general arrangements.

(1) 1·9300 grm. plumbic chloride mixed with 5·1351 grms. phosphoric oxychloride and gradually heated to 127°·5 in the dry air-current for $1\frac{1}{4}$ hour, increased in weight by ·0027 grm. = 00·19 per cent.

(2) 0·7086 grm. sodic chloride with 4·5103 grms. phosphoric oxychloride, heated as before to 120°·5, gave an increase of ·0007 grm. = 00·09 per cent.

(3) 0·8274 grm. potassic chloride and 3·5664 grms. oxychloride, heated for 1 hour to 127°·5, increased by ·0016 grm. = 00·19 per cent.

(4) 1·9680 grm. phosphoric pentoxide was gradually heated in the air-current to 148° during one hour, and increased ·0002 grm. = ·01 per cent.; and this, after mixture with a considerable quantity of two combined preparations of oxychloride and slow heating for ¾ hour to 145° in the air-current, increased by ·0114 grm. = 00·59 per cent.

(5) 1·3018 grm. baric nitrate, heated with an unweighed excess of oxychloride to 123° during 1⅓ hour, acquired ·0006 grm. = 00·05 per cent. The residue contained an infinitesimal trace of chlorine.

I now proceed to the description of individual reactions.

Argentic Nitrate.

The ordinary commercial salt not unfrequently contains copper and some sulphate derived from the incompletely purified hydric nitrate employed in its preparation. The salt actually used in these experiments was made by precipitating argentic chloride from the commercial nitrate, washing repeatedly with hot aqueous sodic acetate, and reducing the chloride by fusion with a mixture of sodic carbonate and chloride. The silver so obtained was dissolved in specially prepared commercial hydric nitrate containing but little sulphate; and the solution, after evaporation to dryness, was fused and crystallized from alcohol. In this way all the ferric nitrate was decomposed and removed simultaneously with the argentic sulphate. Desiccation and crystallization from water completed the purifying process.

This nitrate, as was the case with each of its successors, was first reduced to a powder of moderate fineness; it was then dried at 180° and allowed to remain for some time to cool over oil of vitriol. When brought into contact with the oxychloride, there was a slight increase of temperature; and chemical action was indicated by the now dead-white appearance of the nitrate, the orange coloration of the oxychloride, and the evolution of minute bubbles of gas. At about 40° the action was evidently a maximum, and a tawny gas of rapidly deepening colour came off freely, smelling of aqua regia, and capable of reddening and afterwards bleaching litmus. At this time also numerous light flakes were observed floating in the oxychloride. At about 71° the reaction had apparently terminated; no more coloured gas was expelled, and the liquid resumed its ordinary appearance.

Between 100° and 110° the latter distilled over almost entirely. If the temperature were raised much beyond this point, it was noticed that, at some part of the interval 118°–125°, a little orange-coloured gas would be eliminated. The bare occurrence of this secondary reaction could not always be avoided. A short time after the last drop of the distillate had volatilized under the continued influence of the heated air-current, the bath was removed and the apparatus was cooled, washed externally, dried, detached, closed up, and weighed. The average duration of the operation was $1\frac{2}{3}$ hour. The " residue " had a pale orange colour. When cold water was added to it, a varying amount of heat was evolved, it being observed that this evolution was greatest when the consumption of nitrate had been greatest, and when consequently the effect of the action of water on the phosphoric pentoxide was least masked by conversion into work of solution. The addition of water, moreover, immediately changed the orange tint of the residue into a white; and the aqueous washings always reddened litmus powerfully*.

The analytical treatment of the residues presented no features of special interest†.

	I.	II.	III.	IV.	V.	VI.	VII.
Argentic nitrate employed	2·8568	2·1746	1·0519	2·7633	2·0152	1·8878	2·4959
Residue obtained ..	2·8093	2·1800	1·0589	2·7345	2·0182	1·8631	2·4529
Temperature	125°	121°	122°	120°	115°	115°	118°
Composition of the residue, viz. :—							
Argentic nitrate‡	64·02	40·77	25·96	57·87	54·78	53·57	47·13
Argentic chloride	31·78	49·77	61·91	36·44	38·03	40·31	46·09
Phosphoric oxide	4·30	9·47	12·93	6·60	8·23	7·02	8·49
	100·10	100·01	100·80	100·91	101·04	100·90	101·71
Hence the values of *α* are respectively............	7·32	5·21	4·74	5·47	4·58	5·69	5·38

The mean of these numbers is 5·48; probable error 0·21.

* This latter remark holds true for all the nitrates alluded to subsequently.

† The composition of the residue is expressed in percentages throughout this paper, except in the case of lithic nitrate. This has been done partly for the sake of uniformity, partly for showing sensible aberrations in certain reactions; it need not be remarked that the experimental error in analyses of this kind falls considerably within the common departure of the totals from 100.

‡ Calculated from the original nitrate on the basis of the chloride found.

Plumbic Nitrate.

A specimen of plumbic nitrate obtained by three crystallizations of a commercial sample (the mother-liquor being rejected at each operation) was found to be sufficiently pure. By evaporation with hydric sulphate it appeared to contain 62·61 per cent. lead, theory requiring 62·55 per cent. On account of the tenacity with which this salt retains a small percentage of water, even at 100°, it was powdered finely and heated in an air-bath to 180°–185° during an interval varying from eighteen to forty-two hours. The reaction-tube generally received its supply of nitrate at the latter temperature, after which it was closed immediately and cooled.

On bringing the oxychloride and nitrate together, some slight commencement of action was usually perceptible in the cold. At 60° the liquid became turbid and acquired an orange colour; at 82° a gas was evolved closely resembling chlorine in smell and appearance, but of too light a colour to contain much nitric peroxide. At 100°, however, the tint of the gas had become a deep orange; and at 115° some phosphoric oxychloride emerged from the apparatus, having hitherto been retained, doubtless, by the nitrate, just as water is retained, above its point of ebullition. Hence it was necessary to heat the residue to a higher degree than was required in the case of the silver-salt—an operation which, it was found, could be carried out with safety below 162°. Above this temperature a secondary and different reaction is speedily accomplished. The average time employed in one experiment was about 1¾ hour. The residue had a faint yellow tint, and did not, for the most part, become heated in contact with cold water. But it was several times observed that, when the water reached the exit end of the body of the reaction-tube, considerable heat was produced—an evident indication that the phosphoric oxide had wholly, or almost wholly, collected in that spot.

For analysis, the contents of the tube were transferred to a beaker, and heated for twelve hours with aqueous and hydric nitrate. The argentic chloride so formed was weighed in the usual manner, and calculated as plumbic chloride; the filtrate from it, after addition of ammonia until a precipitate began to form, was treated with ammonic hydrosulphide; and after prolonged repose, the final filtrate was precipitated with magnesia mixture.

The experiments exhibit a varying discrepancy, owing, as is probable, to the action of that portion of the oxychloride which was expelled at 115°, and which, being no longer in excess of the

nitrate, nor confined to its due distilling-point (below 110°), would be likely to affect a converse transformation.

	I.	II.	III.	IV.	V.
Plumbic nitrate employed...	[2·7727	3·9858	2·7045	3·4883	3·1795
Residue obtained	2·7945	4·0138	2·7472	3·5477	3·2260
Temperature	125°	137°	131°·5	130°	130°
Composition of the residue, viz. :—					
Plumbic chloride	10·76	10·09	12·65	8·11	11·07
Phosphoric oxide	2·02	1·67	2·63	1·98	2·20
Plumbic nitrate	86·41	87·29	83·38	88·67	85·37
	99·19	99·05	98·66	98·76	98·64
The values of α are	5·44	6·17	4·91	4·18	5·14

The mean is 5·17 ; probable error 0·19.

Were the equation

$$3\,Pb\,(NO^3)^2 + 2\,POCl^3 = Pb^3(PO^4)^2 + 6\,NO^2\,Cl$$
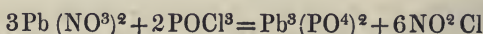
true, under the experimental conditions, the residue would contain no chlorine, it would be neutral to wet litmus, and, in any case, would weigh less than the original nitrate (for a total attack, 0·8534 of the nitrate). The above results, however, agree with neither of these suppositions.

Thallous Nitrate.

The thallous nitrate employed was free from sulphate and thallic salt ; 1·2045 grm. of it yielded 1·1358 grm. sulphate =94·30 per cent. (theoretically 94·72 per cent.). For experiment it was dried for several hours at 156° and cooled in the desiccator.

Phosphoric oxychloride hardly acted on thallous nitrate in the cold ; but at 45° a yellow coloration was observed, which became red at 71°–75°, with turbidity. At 75°–80° the maximum change occurred, and a gas, having the unmistakable colour of nitric peroxide, was given off freely. Soon afterwards the reaction ceased. It was noticed, however, that on raising the temperature to 116°–118°·5, a very small quantity of a greenish gas was evolved, apparently chlorine. Each operation occupied rather more than an hour. The residue had a very pale yellow tint with a white border, and on the addition of water disengaged heat. Its aqueous solution gave a brown precipitate with ammonia. The course of analysis differed in no respect from that pursued in the case of the lead salt.

	I.	II.
Thallous nitrate taken	1·8993	2·1431
Residue	2·0870	2·4041
Temperature	118°·5	116°·5
Composition of the residue, viz. :—		
Thallic dichloride	37·77	42·01
Phosphoric oxide	8·89	9·97
Thallous nitrate.....................	54·46	48·50
	101·12	100·48
Value of α	4·40	4·36

The excess of the above percentages over the proper amount is doubtless owing to the whole of the chlorine found having been calculated as dichloride, whereas, on account of the slight loss of chlorine already referred to, the residue must have contained a little sesquichloride. In the second of these experiments, where the temperature was purposely restrained to the lowest advisable limit, less chlorine was probably missing, and calculation agrees more nearly with fact. Mr. Crookes informs me that, although he has never placed himself precisely in the conditions under which I worked, the behaviour of the dichloride, as here described, is such as he would have been induced to expect.

Sodic Nitrate.

This body was prepared, in a state of satisfactory purity, by the two following methods. In the first, a sample of common sodic chloride was repeatedly digested with strong aqueous hydric chloride assisted by heat; and the insoluble portion, after having been drained, was evaporated with an adequate excess of hydric nitrate which had been distilled below its boiling-point. In the second, some sodic hydrate, prepared directly from the metal, was evaporated with excess of the same hydric nitrate. Either product was dried, fused, dissolved in water, and evaporated to crystallization. Nitrate obtained by the first process was used in experiments I. and II. On evaporation with aqueous hydric chloride it furnished 68·78 per cent. of sodic chloride. Theory requires 68·76 per cent. In experiment III. a salt from the second source was employed. Desiccation was effected with facility at 165°–180° in a few hours. Contact with the oxychloride gave rise to immediate action and perceptible warmth. At 40° the liquid was turbid; at 50°–65° there was very considerable action; and at 70° the contents of the tube had a peculiar fleshy colour and were semisolid. Liquefaction gradually ensued; and complete decolorization occurred at 97°·5. The

average time of each operation was about an hour and three quarters.

When water was added to the residue, the evolution of heat was very decided (being much greater than in the case of the three preceding nitrates), and the residue dissolved entirely.

	I.	II.	III.
Nitrate taken............................	1·0718	1·2278	1·2014
Residue obtained	1·4113	1·6309	1·5694
Temperature	117°	116°	120°·5
Composition of the residue, viz. :—			
Sodic chloride	22·82	23·45	23·38
Phosphoric oxide	33·47	33·62	32·08
Sodic nitrate	42·77	41·19	42·56
	99·06	98·26	98·02
Value of α	1·65	1·69	1·77

The mean of the three quotients is $\alpha = 1·70$.

Potassic Nitrate.

The starting-point in the preparation of this nitrate was hydropotassic tartrate. The tartrate, after two crystallizations, was dried, ignited, and extracted with dilute aqueous hydric chloride (during which process some hydric sulphide was disengaged). On evaporation of the solution and addition of more hydric chloride, potassic chloride was deposited; and this salt, after washing with concentrated aqueous hydric chloride, was dried, and subsequently evaporated with hydric nitrate that had been distilled below the boiling-point. When the preparation was attempted by directly treating the ignited tartrate with hydric nitrate, the product contained sulphate. The sample referred to below contained an infinitesimal trace of chloride, but was otherwise pure; on evaporation with aqueous hydric chloride it yielded 73·70 per cent. potassic chloride, 73·75 per cent. being the number required by theory. The nitrate was dried for several hours at a minimum temperature of 160°.

Phosphoric oxychloride gave rise to immediate action. At 40°–54° the mass in the reaction-tube became flesh-coloured and viscid, apparently giving off chlorine and nitric peroxide as the temperature increased. At about 92° the liquid state was restored, and perfect decoloration took place at 102°. These phenomena are remarkably similar to those observed with the sodium salt. The mean duration of an experiment was nearly an hour and a half.

Special precaution had to be taken in bringing the residue

into contact with cold water, partly on account of the great heat evolved, and partly to avoid loss by intumescence; complete solution took place.

In the following analytical statement the numbers are, as usual, given in percentages; but the amount of nitrate is not recorded*, calculation having shown that it did not even remotely approximate, as heretofore, to the undetermined difference :—

	I.	II.	III.	IV.	V.	VI.
Nitrate taken......	1·4248	1·9584	1·8572	2·0737	1·4156	1·2175
Residue obtained ..	1·9600	2·5770	2·4147	2·7575	1·8103	1·5993
Temperature	115°	116°	116°	115°·5	134°	123°·5
Constituents of the residue, viz. :—						
Potassic chloride.	34·35	30·10	29·09	31·12	28·25	30·72
Phosphoric oxide.	33·10	28·79	27·94	29·69	26·23	29·07
Value of α	1·976	1·990	1·982	1·996	2·050	2·011

The mean of all the values of α is 2·001; the mean, omitting that deduced under V., is 1·991, which is adopted as the most probable value.

Cæsic Nitrate.

A commercial specimen of cæsic chloride, stated to have been prepared from the deliquescent tartrate by Bunsen's method, was evaporated with hydric nitrate and fused. Nitration took place with extraordinary facility; and the fused mass was glassy and fissured, much resembling the fused sodic salt. Solution and filtration had to be resorted to in order to separate a little ferric oxide. Allowing for the weight of this impurity, and of some water which was present, the chloride furnished 116·29 per cent. nitrate, theory requiring 115·76. The salt was dried over oil of vitriol, and then exposed to 180° for $3\frac{1}{2}$ hours. Contact with the oxychloride developed immediate action, accompanied by turbidity. At 56° the mixture became partly gelatinized, with dark orange colour, and emitted an odour of chlorine; subsequently a gradual liquefaction took place. The time

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* In a portion of the dissolved residue in VI. the unattacked nitrate was determined by Harcourt's process (Chem. Soc. Journ. vol. xv.) and found to be 51·73 per cent. Either, therefore, the phosphorus and chlorine could not have been combined as previously, or the nitrogen eliminated in the form of ammonia by this method could not have wholly existed as nitrate. The latter hypothesis seems to be the more probable one. While experimenting to make myself familiar with Harcourt's process, I found that if potassic nitrate be in the presence of hydrate and a small portion of phosphate, the whole of the nitrate is not decomposed in the ordinary manner; the apparent yield, as proved by a mean of five concordant results, requires multiplying by 1·0689. When the phosphate was absent, a normal number was obtained.

occupied in each operation was an hour and a half. Considerable heat was produced on adding cold water to the residue, which speedily dissolved.

	I.	II.
Nitrate taken..........................	1·2617	1·4702
Residue	1·5287	1·7921
Temperature	120°	122°·5
Constituents of the residue, viz. :—		
Cæsic chloride	52·80	53·93
Phosphoric oxide	19·74	20·99
Value of α	2·254	2·166

The mean value of α is 2·21.

It may be mentioned that the ordinary method of determining phosphate, and Rose's process (which eliminates the influence of a foreign metal), were found to agree exactly.

Rubidic Nitrate.

The process of preparation was the same as in the preceding case; and nitration took place readily. The chloride employed yielded 121·56 instead of 121·97 per cent. of nitrate, as demanded by theory. The fused nitrate split violently asunder on cooling, and assumed a somewhat pearly appearance. It was dried over oil of vitriol and at 180°–190°.

Action commenced as soon as the oxychloride was added, an odour of chlorine or aqua regia being very perceptible. At 45° there was a copious evolution of a yellow gas; at 50° the liquid became turbid, and at 87° deposited crystals. The average time occupied in the operation was 1·3 hour. Heat was doubtless produced when water was poured on the residue in every case, but it could only be perceived in III. Solution took place readily.

	I.	II.	III.
Nitrate taken..........................	1·4315	1·0767	2·1349
Residue	1·4901	1·1413	2·4716
Temperature	125°	127°·5	126°·3
Composition of the residue, viz. :—			
Rubidic chloride	11·52	15·61	29·13
Phosphoric oxide	5·18	7·82	15·70
Values of α	2·61	2·34	2·18

The mean value of α is 2·38.

Lithic Nitrate.

This salt was prepared by treating lithic chloride with hydric nitrate that had been distilled below the boiling-point, and otherwise proceeding as in the case of potassic nitrate. In order to ascertain its purity, a quantity was converted into chloride (the nitrate being extremely difficult to weigh for this purpose) by evaporating to dryness with aqueous hydric chloride and fusion. The new chloride yielded 129·52 per cent. of sulphate, theory requiring 129·56; the nitrate from which it was derived was free from sulphate and chloride, and was neutral.

In order to secure as complete a desiccation as possible, the nitrate, which is an excessively hygroscopic substance, was powdered at 100°–180° and rapidly introduced into the reaction-tube; this with its contents was then plunged in an air-bath already heated to 200°, in which state it was maintained for a period of 22–48 hours. Phosphoric oxychloride gave rise to immediate and energetic action; and it became turbid, with deposition of flakes and crystals, at 25°–35°. At 75°–80° the liquid was again clear; at 80°–90° the principal part of the reaction took place; at 100° much (or even all) of the nitrate had dissolved, and oily streaks (a sign of incipient viscidity) were observed in the mixture. On gradually raising the temperature some crystals were deposited, the oxychloride for the most part (but never wholly) distilled away, and the mass in the tube became vesicular, with so much intumescence as to hinder, and finally interrupt, the passage of air. This last result invariably ensued, although great care was taken to attain the ultimate stage very slowly, and the operation was sometimes prolonged to more than five hours.

The tube with the residue was plunged into cold water, excepting on one occasion (experiment I.), it having been found that the ordinary or converse operation was attended with danger of loss owing to the frothing that ensued. Solution took place quietly, and was complete in about an hour, this period being required by the free oxychloride which invariably separated. As the residue might contain lithic nitrate and chloride, phosphoric oxide and oxychloride, and possibly some fifth constituent, it was evident that the value of α would not admit of direct determination unless in a case where the attack was total. Only one of the following experiments (V.) certainly fulfils this condition. Fortunately it was found, on examination of the analytical numbers, that the ratio (r) between the weight of argentic chloride and magnesic pyrophosphate was still sensibly constant. Hence there is a basis of comparison between the instances of total and partial attack. In experiment VII. the amount of residual nitrate was determined by Harcourt's process, and agrees well with

the supposition that the rest of the lithium was combined with chlorine; the weights of phosphoric oxide and oxychloride admit of calculation, and the value of α can be assigned. In I. some of the residue was lost, and the analytical statement, although available for calculating the ratio (r) just alluded to, does not represent each total weight.

	I.	II.	III.	IV.	V.	VI.	VII.
Nitrate taken...............	2·3604	1·2659	1·2648	0·7065	0·5514	0·3315	0·6315
Residue	4·5700	2·7842	2·6715	1·8318	1·7411	0·9710	2·2326
Temperature	121°	128°	130°	134°	140°	133°·5	119°
Composition of the residue, viz.:—							
Lithic chloride	—	—	—	—	0·3396	—	0·3431
Lithic nitrate...........	—	—	—	—	none	—	0·0723
Phosphoric oxychloride	...	...	...	...	·7178	...	1·0327
Phosphoric oxide	...	...	...	...	·6919	...	·7214
Argentic chloride	[2·7765]	4·3458	3·8380	3·0084	3·1599	1·7295	4·0548
Magnesic pyrophosphate.	[1·4077]	2·2356	2·1720	1·7832	1·6008	·8964	1·8752
r	1·97	1·94	1·77	1·69	1·97	1·93	2·16
α	—	—	—	—	1·64	—	1·59

Mean value of r (V. and VII.) . . . . 2·06
,, α ,, ... 1·61
,, r (I., II., III., IV., and VI.)   1·86

In the last group of experiments r closely approaches its value in the first group, where α is known. The value of α may therefore be taken generally as 1·61. The mean of all the determinations of r is 1·92; and it can easily be shown by means of the data furnished in experiment V., that the residue, so far as it had taken part in the reaction, had a composition nearly to be represented by the following formula:

$$3\,\mathrm{Li}\,\mathrm{Cl}, 2\,\mathrm{PO}\,\mathrm{Cl}^3, 2\,\mathrm{P}^2\,\mathrm{O}^5,$$
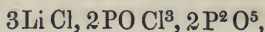
which gives $r = 1·94$ and $\alpha = 1·50$.

Subsidiary Operations.

In order to obtain some information as to the nature of the gaseous products of the action of phosphoric oxychloride on nitrates, it was manifestly necessary to select a nitrate upon which that reagent has considerable action. The theoretical excess of oxychloride, indicated by the equation

$$3\,\mathrm{Pb}\,(\mathrm{NO}^3)^2 + 2\,\mathrm{PO}\,\mathrm{Cl}^3 = \mathrm{Pb}^3\,(\mathrm{PO}^4)^2 + 6\,\mathrm{NO}^2\,\mathrm{Cl},$$
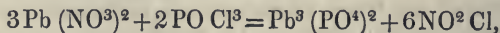
could not be employed, inasmuch as that quantity is insufficient to cover the solid salt, and another well-known reaction was therefore to be apprehended from the altered chemical conditions due to purely mechanical circumstances. The fact that about

85 per cent. of plumbic nitrate usually escapes the reaction un-injured, prevents moreover the useful employment of that compound. Argentic nitrate was therefore selected.

30 grms. of argentic nitrate and 23 cubic centims. oxychloride were placed in a bulb, which communicated by means of an elongated neck with three U-tubes. The first of these was of small size and immersed in cold water; the second was larger, and covered with ice and salt; the last and remotest was plunged in a mixture of ice and hydrous calcic chloride, the temperature of which was $-35°$. The interior of this apparatus had been carefully dried. The bulb was placed in a water-bath, the heat of which was raised gradually. On observing the phenomena presented during the reaction, it was found that they sensibly agreed with what have already been recorded in this paper. A very little oxychloride escaped from the bulb-apparatus, and was retained by the first two U-tubes. In the third U-tube about 1·5 cubic centim. of a tawny distillate had collected; its vapour-density, as determined by Dumas's process, was found to be 1·981 at 99°·6. Assuming it to be a mixture of nitric peroxide and nitrylic chloride, the composition and volume of the vapour at that temperature were as follows:—

$$\begin{array}{lcc} & & \text{cub. centims.} \\ \text{Nitric peroxide} & . \quad . & 99·6 \\ \text{Nitrylic chloride} & . \quad . & 35·5 = 0·0414 \text{ grm. chlorine.} \\ \hline & & 135·1 \end{array}$$

This result was confirmed by determining the chlorine in the vapour-density flask; its amount was found to be 0·0436 grm. The powerful frigorific means employed did not suffice to retain all the gaseous products of the reaction. A pungent and pale yellow vapour escaped continually from the apparatus; but although it reddened and bleached moist litmus, it was quite unlike chlorine in odour. This body was arrested by aqueous aniline hydrochloride; but another and colourless gas still escaped absorption.

The strict determination of the presence or absence of free chlorine under the conditions here indicated did not appear to be a surmountable problem. I contented myself, therefore, with the above results, which point conclusively to the evolution of (at least) nitric peroxide, nitrylic chloride, and most probably oxygen.

Discussion.

(1) By selecting and employing the same unit of measurement throughout namely the common chemical unit (P^2O^5) of phosphoric pentoxide, it appears that the nitrates are con-

stantly unlike in their power of producing a residue in which chlorine and that oxide are relatively retained. The numbers to which α corresponds are ratios; and the formal process whereby they are obtained may therefore be termed "the method of ratios." It must be distinctly understood that α is a ratio and not an absolute quantity; it may be compared to interest, while the nitrate corresponds to a stock from which interest has been derived. The dynamical *use* that can be made of a nitrate is represented by the symbol α. The data from which α has been deduced (namely, certain weights of argentic chloride and magnesic pyrophosphate) are, if singly considered, new with each experiment; they depend on time, rate of heating, the state of division of the nitrate, and on the incidence of other and even minuter conditions. But assuming the results to have been brought about under a law of chemical action, the value of α must be independent of those circumstances, which could only *pari passu* affect the primitive numerator and denominator; it is essentially related only to the actual occurrence of the reaction. This is very well shown in the case of baric nitrate, from which neither chloride nor phosphate was obtained, and hence $\alpha = 0$, in accordance with which result it is found that no other substance was formed; in other words, no reaction occurred.

In the following Table Σ stands for the symbolic value (atomic weight) of a nitrate, and $Q = \dfrac{\Sigma}{\alpha}$.

		α.	Σ.	Q.
⎧ Thallous nitrate	. .	8·76	265·30	30·29 ⎫
⎨ Argentic ,,	. .	5·48	169·94	31·01 ⎬
⎩ Plumbic ,,	. .	5·17	165·56	32·02 ⎭
Rubidic ,,	. .	2·38	147·40	61·93
Cæsic ,,	. .	2·21	195·01	88·24
⎧ Potassic ,,	. .	1·99	101·14	50·82 ⎫
⎩ Sodic ,,	. .	1·70	85·05	50·03 ⎭
Lithic ,,	. .	1·61	69·00	42·86

With regard to the above Table, I may remark that it complies with the logical requirement of a common series by referring all the nitrates to the same weight of oxynitryl ($N\dot{O}^3$); for the salts employed have only been considered as nitrates, and not in any other relation. Hence the value for plumbic nitrate is $\dfrac{Pb(N\dot{O}^3)^2}{2} = 165\cdot56$. Again, by reference to the analysis of the thallic residue, it will be observed that in that particular case a dichloride has been produced instead of a monochloride as in the other cases. Hence thallous nitrate has simulated thallic dinitrate. But the formula of the nitrate $Tl(NO^3)^2$ corresponds

to the double formula of thallous nitrate, or $Tl^2(NO^3)^2$; and $Tl^2(NO^3)^2$ cannot be admitted into the series Σ, because it contains $(NO^3)^2$ instead of (NO^3). Since, then, we can only take into account one half of $Tl^2(NO^3)^2$, the actual symbolic value, it becomes necessary to represent the double energy of thallous nitrate by doubling the experimental value of α, which then becomes 8·76. This argument is justified by the natural position in which it places thallous nitrate—that is, near to argentic and plumbic nitrates.

(2) The exact significance of α is not difficult to ascertain. When one nitrate surpasses another in the power of fixing chlorine per unit of phosphoric oxide, that is an index of superior chemical activity, or, to name the ultimate cause, attraction. When this attractive effect can be specified for several nitrates in a series, thereby valuing the choice of the oxychloride, it is shown that the phenomenon is of that kind which Bergman termed "elective attraction." Hence α is the coefficient of elective attraction, or elective coefficient of the nitrates.

At first sight it might appear that a coefficient of this kind may be evaluated with equal propriety on the basis of phosphoric oxide formed per unit of chlorine fixed. But if we were to adopt this supposition, we should find the following results. The value of the coefficient in the case of lithic nitrate would be 0·62, in the case of thallous nitrate 0·11. Now, although α depends essentially on the mere occurrence of the reaction, yet, in forming this *à priori* estimate of it, we must be guided by the general account of chemical change effected. A reference to the tabulated results will show that as much as about one half the thallous nitrate became active in one experiment; in another the whole of the lithic nitrate became active; and these are the extreme terms of the series. Suppose these (absolute) weights are to each other as 1 : 2, and compound them (1) with the ratio 8·76 : 1·61 (say, 5 : 1), and (2) with the ratio 0·11 : 0·62 (say, 1 : 5). We have in the former case the relation 5 : 2, in the latter 1 : 10. Bearing in mind the fact for which we have to provide, that *any* nitrate in the series might become wholly active[*], which would make the ratio of absolute weights 1 : 1, it is clear that we must select that construction for α which causes its value, when compounded with the ratio of such absolute weights as have been selected, to approach the more nearly to 1 : 1. Now 5 : 2 is nearer to 1 : 1 than is 1 : 10; and the mode of evaluating α adopted in this memoir is therefore more reasonable than the inverse method.

In order to render my argument more plain by illustration, I shall recur to the conception of stock as already mentioned. Sup-

[*] Compare *Argentic nitrate,* exp. III.

pose there are two stocks, A and B; A (originally 100) sells for 35, and is a 5 per cent. stock; B sells for 70, and is a 2 per cent. stock. Now although interest, we may say, depends on the mere occurrence or negotiation of the loan, yet, in forming an *à priori* estimate of it, we must be guided by the fluctuations of loans generally. In the case of A little more than one third of the original price is the basis of a transaction, in the case of B rather less than three fourths. The actual absolute prices are to each other A : B :: 1 : 2. Now we might value the stocks by their percentage return, namely, A at 14 and B at 2·8; or we might value them by 100 × the reciprocals of these numbers, that is, A at 7 and B at 35. In what manner can we decide as to the validity of either method? By first supposing both stocks to rise to par, and multiplying each by its own rate of interest, A : B :: 500 : 200, we find that A is worth 5 : 2 times B, in accordance with the common estimate. When A and B are at 35 and 70 respectively, we have, by a similar comparison,

$$A : B = 35 \times 14 : 70 \times 2·8 = 5 : 2 ;$$

but, by the use of the reciprocals already mentioned,

$$A : B = 35 \times 7 : 70 \times 35 = 1 : 10.$$

The latter result is manifestly false. The former is correct; because it coincides with the principle underlying such transactions, that all stocks tend to have more nearly the same rate per cent. as they approach par. Now this principle, if we make due allowance for the nature of an analogy, is identical with the one employed in deciding as to the construction of the ratio α. The low price of a stock corresponds with the sluggishness of a nitrate to enter into reaction, as especially instanced in plumbic nitrate; the high price of a stock corresponds with the tendency for the whole mass of a nitrate to enter into reaction, as instanced in lithic nitrate. A high-priced stock pays a low rate of interest; a wholly reactive nitrate has a low elective coefficient. I repeat in this place my former statement, that the elective coefficient α represents the dynamical *use* that can be made of a nitrate. See also (7).

(3) In the silver group the mean value of Q is 31·11, and the following equation may be accepted therefore:—

$$\alpha = \frac{\Sigma}{31·11}.$$

Similarly, in the potassium group we have

$$\alpha = \frac{\Sigma}{50·42}.$$

Hence within each set of nitrates *the elective coefficient is directly*

proportioned to symbolic value. It is further sufficiently apparent that (excepting rubidic nitrate) α and Σ increase and diminish in the same general order.

Assuming the correctness of this law, and a possible small interference with it for each nitrate, the occurrence of somewhat wide variations from the mean value of α in the case of plumbic and argentic nitrates, while in the case of nitrates of lower symbolic value that variation is very small, meets with an evident and satisfactory explanation.

(4) The quotients Q represent the weights of nitrates which correspond to the unit of elective attraction. They are therefore strictly *equivalent* numbers in the legitimate sense of that term. Within the limit of experimental error they constitute an incomplete arithmetical series, the most probable value of whose first term, as determined by the method of least squares, is 6·258; so that $Q=m6\cdot258$, m being integral. Such a series is already known to chemistry. The following remarks*, containing the first announcement of its existence, are evidently applicable to my own experiments, which were very mainly concerned with the diffusion of a solid nitrate in liquid phosphoric oxychloride. "The fact that the relations in diffusion of different substances refer to equal weights of those substances, and not to their atomic weights or equivalents, is one which reaches to the very basis of molecular chemistry. The relation most frequently possessed is that of equality, the relation of all others most easily observed. In liquid diffusion we appear to deal no longer with chemical equivalents or the Daltonian atoms, but with masses even more simply related to each other in weight. Founding still upon the chemical atoms, we may suppose that they can group together in such numbers as to form new and larger molecules of *equal weight for different substances*, or, *if not of equal weight, of weights which appear to have a simple relation to each other*. It is this new class of molecules which appear to play a part in solubility and liquid diffusion, and not the atoms of chemical combination." The next step in this direction was taken by Chiżyński†, who performed a series of remarkable experiments on the fractional precipitation of mixed calcic and magnesic chlorides by means of a phosphate. He found that "chemical action is proportional to the product of the chemical masses into their coefficients of affinity," and that "equal masses of calcic chloride and magnesic chloride have equal, but oppositely active, coefficients of affinity." Here, then, we have a third group of salts, in which, as chemical evidence shows, the real or dynamical "equivalent" is an "equal weight."

* Graham, Phil. Trans. 1850, p. 46.
† *Ann. Chem. Pharm.*, Supp. iv. p. 226.

(5) If the energy of elective attraction be directly as symbolic value, it ought to vary inversely as specific heat.

According to a carefully prepared Table given in Lothar Meyer's *Die modernen Theorien der Chemie* (p. 48), the mean value of the product of symbolic value into specific heat (excluding the usual anomalies) is 6·246. The identity of this number with the first term in the Q series is unmistakable. Again, in a large majority of cases, if s stand for specific heat, $\Sigma s = n\,6\cdot25$, n being integral. I find that in my results m is greater than n. Let $m = xn$. Then

$$Q = xn\,6\cdot25,$$

and

$$\Sigma s = n\,6\cdot25 ;$$

$$\therefore\ Q = x\Sigma s,$$

and

$$\alpha = \frac{\Sigma}{Q} = \frac{\Sigma}{x\Sigma s} = \frac{1}{xs},$$

the expression for the energy of elective attraction in terms of specific heat. Comparing the coefficients (α, α') for any two nitrates, the following relations are obtained:—

$$\frac{\alpha}{\alpha'} = \frac{m'}{m}\frac{\Sigma}{\Sigma'} = \frac{x's'}{xs}.$$

Where $m = m'$ and $x = x'$, we have the simple expression

$$\frac{\alpha}{\alpha'} = \frac{\Sigma}{\Sigma'} = \frac{s'}{s}.$$

The following instances may be adduced in verification of the latter case:—

$$\frac{\alpha\ \text{(potassic nitrate)}}{\alpha\ \text{(sodic nitrate)}} = 1\cdot17 \text{ by the method of ratios,}$$

$$= \frac{\cdot2782}{\cdot2388} = 1\cdot17 \text{ by specific heat;}$$

$$\frac{\alpha\ \text{(thallous nitrate)}}{\alpha\ \text{(argentic nitrate)}} = 1\cdot59 \text{ by the method of ratios,}$$

$$= \frac{\cdot1433}{\cdot0942} = 1\cdot53 \text{ by specific heat.}$$

In the case of the other nitrates, which require the previous formula, I find the verification to be equally satisfactory. If the reader should desire to perform the calculation, he will observe that $n = 3$ for plumbic nitrate, and $n = 4$ for the other nitrates; $\Sigma s = 25\cdot00$.

(6) The experimental values of α lie between the limits in-

dicated by the following equations, the bracketed portions indicating potential (probably not actual) substances :—

$$\alpha = 6\cdot0 \quad 6\,MNO^3 + 2\,POCl^3 = 6\,MCl + P^2\,O^5 + 3\,[N^2\,O^5],$$

$$\alpha = 1\cdot5 \quad 6\,MNO^3 + 8\,POCl^3 = 6\,MCl + 4\,P^2\,O^5 + 6\,[NOCl^3].$$
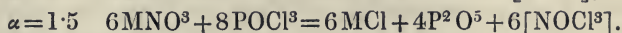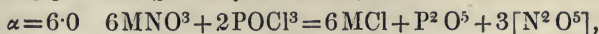
By taking the first equation a times and the second b times, the elective coefficients may be reconciled with the whole numbers required by common chemical equations.

The value of M in the silver group, when $\alpha = 6\cdot0$, would be 125·5, and for $\alpha = 1\cdot5$ an analogue of lithium would have the symbolic value 4 nearly.

(7) That the intensity of elective attraction is proportional to symbolic value, is written explicitly upon every page of the history of chemistry. I shall merely draw attention to a few out of hundreds of examples that might be adduced to corroborate the correctness of the law, and the fact that chemists have long unconsciously been guided by its results.

When argentic nitrate is added to a dilute aqueous solution of potassic iodide, bromide, and chloride (these salts having been mixed in any proportion), argentic iodide, bromide, and chloride are successively precipitated, that is, in the order of their symbolic values. Argentic chloride may be wholly converted into bromide by digestion with aqueous potassic bromide ; and argentic bromide is completely transformable into iodide by aqueous potassic iodide. These comparisons are extremely fair of their kind. The comparison of hot free chlorine with iodine weakened by combination with silver is of course not a fair comparison.

In the formation of salts, baryta has the preference over strontia, and strontia over lime.

In the fractional separation of the volatile members of the fatty series $C_n\,H_{2n}\,O_2$ by Liebig's process, the law is strictly observed, excepting in the case where $n = 2$, which is an intelligible anomaly. Heintz's method of separating the non-volatile members of the same series by means of magnesia exhibits the same order.

In a mixture of the hydrocarbons $C_n\,H_{2n-6}$, benzol is the last to be chlorinated or nitrated.

Warington has shown the superiority of ferric over aluminic oxide as an absorber of alkalies in soils.

In cases of jaundice, taurocholic acid is destroyed in the system before glycocholic acid.

It was the aim of the distinguished Bergman to unite all chemical substances in a series according to the principle of elective attraction. A great part of his life was passed in experimenting qualitatively with a view to that purpose ; and he left on record

an earnest desire that numerical elective coefficients might one day be obtained. I submit to chemists the method of ratios as one out of several means of accomplishing that end. The work which Bergman commenced, and which has been now so long intermitted, may honourably occupy and well be concluded in our own time. It will have its results in a registration of actual or dynamic equivalents, and in the reform of a symbolic system which is every day becoming more disparate from experiment.

My best thanks are due to Sir C. Taylor, Bart., for the use of his laboratory.

LXII. *On Recurrent Vision.* By A. S. Davis, *M.A.*[*]

THE following curious phenomenon has not, I believe, been noticed before. If the end of a piece of charcoal be made red-hot in a flame and then waved about in the dark so as to describe an ellipse or circle a few inches in diameter, a blue image of the burning end is seen following the charcoal at a short distance behind it. The space between the charcoal and its blue image is as dark as the surrounding space. The phenomenon is much better seen if the charcoal be made bright by being blown upon. The interval of time at which the sensation of blue light succeeds the primary sensation at any point of the retina is about a fifth of a second. This may be ascertained by noticing that when the charcoal is moved round in a circle at the rate of about 100 revolutions per minute, the blue image follows the primary image at a distance of about one third of the circumference of the circle.

In seeking for an explanation of this phenomenon, it appeared probable that it was related to another phenomenon lately observed by Professor C. A. Young, and named by him "recurrent vision "[†].

The phenomenon observed by Professor Young is as follows :—When the objects in a room are lighted up by a spark from a powerful electric machine (care being taken to screen the eye from the direct light of the spark), it is observed that the illumination is not single, but that the objects appear to be lighted up two or three times in rapid succession. Professor Young found that the interval between two successive illuminations is about the fifth of a second. He also ascertained that the phenomenon is a subjective one. He does not, however, appear to have noticed whether the colour of the recurrent image differs from the actual colour of the object.

It appeared to me probable, from the experiment with the burning charcoal, that there would be such a difference of colour; and the experiments I am about to describe prove that there is.

Not having a powerful electrical machine at hand, I contrived the following apparatus for producing an instantaneous illumination, and thus exhibiting the phenomenon of recurrent vision.

In a board about 3 feet long and a foot wide, at about a foot from one end of it a rectangular hole was cut 3 inches in the direction of the length of the board and 5 inches in the direction of its breadth. Upon this board another, smaller board was made to slide and act as a shutter to the hole. In this shutter a hole was made similar to the hole in the large board, so that when the shutter was partially drawn up the two holes coincided. A strong elastic band was attached to the board and the shutter in such a manner that, when the shutter was raised, the band acted upon it to pull it down, in the same way as that in which a bowstring acts upon an arrow. A thick rug was nailed round the edges of the board; and when this was thrown over the head, a dark space was formed which could be momentarily illuminated by drawing up the shutter and letting it spring back.

The following experiments were made :—

I. The hole in the board being turned towards the objects in a room and the shutter being drawn up and let go, a recurrent image of the objects was seen; but the illumination was in general too feeble and the impression too momentary for the eye to judge of the colours of the objects. When, however, a bright coloured object was placed in a strong light, the colour of the recurrent image was seen to be different from the actual colour of the object. By gaslight or feeble daylight the recurrent image appeared twice if the object was white, or nearly white. The recurrent colour of a white object is of a blue tint.

II. Various coloured glasses were placed before the aperture, and the board was turned towards the sky.

With a deep-blue glass the recurrent image was a greenish yellow. With a green glass and with a yellow glass it was a reddish blue. With a single red glass, which gave an orange-red light, it was a red-blue.

With two red glasses superposed, which produced a pure red light, no recurrent image was seen, however bright the light.

In the case of the blue, green, and yellow glasses the effect was much better seen when the intensity of the light was moderated by placing against the hole, along with the glass, one or two sheets of white paper; the recurrent image was stronger compared with the primary image, and the interval of comparative darkness between the two images was more clearly perceived,

when this was done.   With the red glass, on the other hand, the recurrent image was only seen when the light was strong.

III. When, instead of producing a momentary illumination, the shutter was raised so that the two holes coincided, and after being held for a short time was let go, the image of the hole became for an instant before disappearing of the same colour as its recurrent image; but in this case there was no interval of darkness before the change of colour took place.   We may conclude from this experiment that the induced excitation, which, when the illumination is momentary, gives rise to a recurrent image, lasts as long as the light which produces it, beginning and ending a fifth of a second after it; but being much feebler, it is only seen after the primary light has disappeared.

IV. The shutter being removed, a large piece of black cardboard with a hole in the middle of it, about half an inch in diameter, was moved quickly about before the hole in the board. A recurrent image of the hole followed the primary image in the same way as in the experiment with the burning charcoal.   When the coloured glasses were placed before the hole, the colours of the recurrent images were the same as in the previous experiments with the same glasses.

The complementary colours of the coloured glasses were ascertained by fixing small pieces of white cardboard against them and holding them up to the light.   The white cardboard took by contrast the complementary colour of the glass against which it was fixed.   In this way it was found that the complementary colour of the blue glass was yellow, of the green glass a blue-red, of the yellow a blue, and of the red a blue-green.   It thus appears that, with the exception of the red glass, the recurrent colour does not differ much from the complementary colour.

The recurrent image given by white light is, as I have already remarked, of a blue tinge.   It follows that the less saturated any colour is the bluer will be its recurrent colour; for a colour which is not saturated may be regarded as a mixture of white light and a saturated colour.   This explains the fact that, though the recurrent colour of a deep-blue glass is a greenish yellow, yet the recurrent colour of a blue object sufficiently light-tinted to give a recurrent image is of a blue tinge.   In fact all light-coloured objects give a recurrent image of a more or less blue tinge; for they all differ but little from white.

A recurrent image of an object may also be produced without any apparatus whatever.   To do this, place the right hand over the eyes so that the palm of the hand covers the right eye and the fingers the left eye.   If the middle finger be then raised for a moment so as to admit light for as short a time as possible into the eye, a recurrent image of any light-coloured object held

against a dark background may be seen. The effect is much better seen by twilight or gaslight than in full daylight. The phenomenon, however, is by no means so well observed by this method as by means of a board and shutter, owing probably to the illumination of the retina not being sufficiently instantaneous.

Professor Young, in explanation of the phenomenon noticed by him, suggests the idea that a nerve-current from the eye to the brain may, on reaching the brain, suffer partial reflection back to the eye, and thence again to the brain, and thus give rise to a second sensation. It being now, however, ascertained that the colour of the recurrent image is entirely different from the colour of the light which produces it, this explanation appears no longer tenable.

If we admit the truth of Dr. Thomas Young's theory of colour-sensation (namely, that the sensation of light is produced by the excitation of three different kinds of nerves,—an excitation produced in one kind giving rise to the sensation of blue light, in another to that of green light, and in a third to that of red light), the above experiments appear to lead to the conclusion that when any one of the three kinds of nerves is excited at any part of the retina, an excitation is induced in those nerves of the other kinds which have their extremities in the same part of the retina. A slight difference between the recurrent and the complementary colour might arise from the mutual action between two kinds of nerves, differing in intensity for different kinds of nerves.

Thus the fact that the recurrent colour given by blue light is rather greener than the complementary colour of the same light, may arise from an excitation in the blue-light nerves inducing a stronger excitation in the green-light nerves than in the red-light nerves. The great difference in the case of orange-red light between the recurrent colour, which is red-blue, and the complementary colour, which is sea-green, is, I believe, explained thus. It was noticed that pure red produces no recurrent image. Hence, when orange-red light, which consists chiefly of red and partly of green light, is used, only the green component gives rise to a recurrent image. Hence the colour of the recurrent image should be the same as that obtained with the green glass. It is, however, rather redder; and this, I think, arises thus: when a red glass is used the light must be intense in order that any recurrent image may be seen; and when the light is intense it continues for a short time after the shutter is closed, giving rise to what is known as a persistent image. This persistent image is superimposed upon the recurrent image and reddens it. With any other colour, the persistent image, if there is any, is

so much feebler than the recurrent image that it produces no alteration in its tint.

In conclusion I would remark upon the apparent analogy between the phenomenon of recurrent vision and that of induced currents in electricity. A nerve-current in one kind of nerves appears to induce nerve-currents in the other kinds in a manner analogous to that by which a current of electricity in one conducting wire induces currents in parallel conducting wires.

Leeds Grammar School.
November 9, 1872.

LXIII. *On the Theory of Electrodynamics.* By M. Helmholtz[*].

THE theory of electrodynamic actions, besides its immediate value for the understanding of this important and prolific branch of physics, is more universally interesting in its relation to the fundamental principles of general mechanics. All the other known actions at a distance can be easily and completely reduced to attractive and repulsive forces of points of masses, while the intensity of these forces depends only on the reciprocal distances of the points and not on their motion. Moreover the hitherto known actions between molecules can either be entirely referred to such forces, or at least are so similar in their whole manner of appearance to the effects produced by gravity that we find no difficulty in imagining them the effects of forces similar in character. But the electrodynamic forces constitute an exception. They form a class of distant actions produced only by the state of motion of the efficient agent, the electricity,—a state of motion which makes itself perceptible as such by a whole series of phenomena—by development of heat in solid conductors, chemical decomposition in liquid conductors, &c. The real laws of the manner of appearance of these forces are, in the main, well known, and have been reduced by F. E. Neumann, Sen., to a comparatively simple expression, which, however, gives not the action of mass-point upon mass-point, but of one linear element of a current upon the other. I have myself given to Neumann's expression of the potential a more general form[†], in which it embraces also the differing expressions resulting from the theories of W. Weber and Maxwell for the potential of each two current-elements. For closed currents all these expressions give the same results ; on the contrary, for open ones, the actions of which have, indeed, at present been little investigated, they

exhibit differences. The plan of my memoir was principally to seek out those differences which it might be possible to discover by practicable experiments. It must here be remarked that the various potential-expressions which I formed differ from one another only by a constant (in my memoir, denoted by k). We obtain Neumann's expression if we put $k = +1$, Maxwell's if $k = 0$, W. Weber's if $k = -1$. The investigation showed that the expressions with k negative led to impossible consequences—namely, to an unstable equilibrium of the electricity in conductors, which, once disturbed, might give rise to infinitely great current-intensities and unlimited charges. On the other hand, the expressions with k positive, or with $k = 0$, gave stable equilibrium, and, even for open currents, only such differences as, with our present experimental means, can hardly be detected; so that what is yet doubtful in the mathematical conception of the law, viz. the value of the constant k, appears to have no effect in the application of it to experiment.

These expressions for the potential of each two current-elements, however, are manifestly not elementary expressions of the last acting forces; for they lead, if we imagine each current-element as a solid body, to at least two forces for each, or to a force and a pair of forces; and the quantity and partly the direction of these forces depend not merely on the situation of the elements, but also on the velocity of the electric currents. The phenomena of induction are only indirectly derived from the electrodynamic potential, through the interposition of the law of the conservation of energy.

Among the further-penetrating hypotheses which seek to ascertain the elementary forces that lie at the base of electrodynamic phenomena, two especially must be mentioned. Mr. Clerk-Maxwell drops the assumption of action at a distance, and assumes that all magnetic, electrostatic, and electrodynamic actions are translated to a distance by the propagation of molecular motions and forces in an elastic medium which fills space. As the theory finally gives for this medium the capability of executing oscillations which are perfectly similar to those of light and have also the velocity of propagation of light, this medium must be identified with the luminiferous æther. It is true that, for the reciprocal action of neighbouring volume-elements of this medium, he assumes laws considerably different from those of the elastic bodies known to us; but he has shown that a kind of reciprocal action, such as he attributes to the æther, can indeed be produced by a mechanical combination of solid elastic bodies. For this purpose a system of cells with elastic walls and cylindrical cavities must be taken, in which elastic balls can rotate and be flattened out by the centrifugal force. In the walls of

the cells there must be other balls, of invariable volume, as friction rollers. These would rotate freely; but their centres of gravity, in insulating media, would merely be displaced by elastic yielding of the cell-wall; in conducting media, on the contrary, at every displacement they must suffer a resistance similar to friction in a viscous liquid. The transference of motion between these balls would be effected only through the adhesion of their surfaces to one another. Displacement of the last-mentioned balls gives dielectric polarization of the medium; streaming of the same, an electric current; rotation of the elastic balls corresponds to the magnetizing of the medium, the axis of rotation being the direction of the magnetic force.

Now, although the idea of such a molecular structure of the space-filling æther may be repugnant to our imagination as too artificial, yet the hypothesis of Maxwell appears to me very important on this account—because it proves that there is nothing in electrodynamic phenomena to compel us to attribute them to an entirely anomalous sort of natural forces, to forces depending not merely on the situation of the masses in question, but also on their motion. Indeed, out of the assumption of those reactions of the volume-elements of the æther upon each other which Mr. Maxwell has assumed, a complete and mathematically very elegant theory of all electric phenomena (magnetic, electrodynamic, and induction) can be developed; and the same theory also gives an account of the phenomena of light.

On the other hand, M. Weber's theory derives the explanation of electrodynamic actions from distant forces of a peculiar kind, acting between the points of the electrical masses, and depending simultaneously on the distances and the relative motions and accelerations of each pair of points. It gives comparatively simple explanations of electrodynamic attractions and of the induction-effects in linear conductors; and its analytic deductions accord perfectly, for all the phenomena to be observed in closed linear currents, with the consequences of the potential-law derived by F. E. Neumann from the phenomena. On this account, Weber's theory (which preceded Maxwell's) was very favourably received, especially by the German physicists. It had, and moreover retains, decidedly the merit of every acute and original thought which endeavours to strike out new paths in science when the old ones appear to lead into an inextricable thicket. I hardly need here remark that the value of such an attempt, if it was sufficient for the state of knowledge at the time, is not diminished when, after twenty-five years' progress of science, the impossibility is shown of carrying it out. Even then such an attempt has not been fruitless. A reconnaissance of unknown ground lying beside the road hitherto kept, if car-

ried out carefully and intelligently, retains its value even if it should only teach that no way exists except the high road.

It was through Weber's hypothesis that a question of the highest significance for the principles of natural science was for the first time tested in the problems of facts, viz. whether elementary forces, incapable of further analysis, must be assumed dependent not merely on the position, but also on the motion of the acting points. In my work ' On the Conservation of Force,' I had stated that forces which depend only on the distance and the velocities, and therefore only on the coordinates of the points, and on their first differential quotient, must necessarily infringe the universal natural law of the conservation of energy, which law proves everywhere true also in electrodynamic phenomena. At that time, however, I had not considered this still more complicated case set up by the Weberian law, in which the forces depend on the coordinates and on the first and second differential quotients ; and this case is certainly compatible with a somewhat extended form of the law of the conservation of energy. If we, as has always hitherto been done, name *vis viva* or *actual energy* the sum of the moved inert masses multiplied each by half the square of its velocity, then, in the usual form of the law, the quantity which I have called *quantity of tension-force*, and the English physicists *potential energy*, is a function of the coordinates of the moved points only ; and the law of the conservation of energy affirms that the sum of the actual and potential energy remains constant in every motion of a mass-system not influenced from without.

If, however, under the action of external forces a self-repeating cyclical process takes place, at the end of which all the points of the system have exactly the same position, and the whole the same *vis viva*, as at the beginning, the sum of the work received from without and the work given out must be equal to zero, so that by no repetition of the process can work be permanently gained or destroyed. If the former were the case, there would be possible a perpetually continuous gain of work without a progressive alteration of the mass-system, and a *perpetuum mobile* might be constructed.

Weber's extension of the law of energy makes also the value of the *potential energy* a function not merely of the position, but also of the velocities of the mass-points. Under this assumption also, by no cyclical process which brings back not merely all the masses of the system to their initial positions, but also each one to its initial velocity, can more work be given out than is received from without, because those quantities of actual and potential energy which constitute the measure of the work are the same at the end of every such cyclical process as at the beginning.

Under these circumstances, however, the values of the forces must necessarily contain second differential quotients of the co-ordinates, because the sum of the force-components corresponding to the individual points and axes of coordinates, each multiplied by the corresponding component of the velocity, must be equal to the differential quotient of the potential energy, taken according to the time ; and the latter, under the condition presupposed, necessarily contains also the second differential quotients of the coordinates according to the time.

In relation to complete cyclical processes, M. W. Weber* has proved that his assumption concerning the value of the electric forces admits no production of work without a corresponding expenditure of forces capable of producing it.

In another place, applications which I endeavoured to make of the differential equations deduced by Kirchhoff from Weber's assumption had led me to the discovery that they corresponded to a state of unstable equilibrium of the electricity in conductors, and that, according to them, currents might be developed which would lead to infinite current-intensities and infinite electric densities.

Replies by MM. W. Weber and C. Neumann have induced me to resume and generalize these investigations, the results of which 1 will here briefly lay before the Academy†.

If we have any number, however great, of mass-points, the inert mass of which may be denoted by μ_n, and all or some of which contain *quanta* of electricity, which, measured according to electrostatic measure, may be denoted by e_n—if, further, r_{nm} is the distance between the points n and m, and q_n the resulting velocity of the point n, ϑ_{nm} the angle which it makes with the direction of the line r_{nm} prolonged beyond n, then the value

(1) of the electrostatic potential

$$P = \tfrac{1}{2} \Sigma \Sigma \left[\frac{e_n \cdot e_m}{r_{nm}} \right];$$

(2) of the electrodynamic potential

$$Q = \frac{1}{cc} \Sigma \Sigma \left[\frac{e_n \cdot e_m}{r_{nm}} \cdot q_n \cdot q_m \cdot \cos (\vartheta_{nm}) \cdot \cos (\vartheta_{mn}) \right].$$

We put, further, the quantity

$$p_n = \frac{1}{cc} \Sigma \left[\frac{e_m}{r_{nm}} \cos^2 (\vartheta_{mn}) \right],$$

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* " Electrodynamische Maassbestimmungen, insbesondere über das Princip der Erhaltung der Energie," *Abh. d. math.-phys. Classe der Königl. Sächsischen Ges. der Wissenschaften,* 1871. The value of the potential was given by the same author in Pogg. *Ann.* 1848, vol. lxxiii. p. 229.

† The investigation will be published entire in the *Journal für reine und angewandte Mathematik.*

and let V denote the potential energy of the remaining forces which act upon the inert masses; then the equation which, in Weber's sense, expresses the conservation of the force becomes

$$\tfrac{1}{2}\Sigma[(\mu_n - p_n e_n)q_n^2] + \mathrm{P} + \mathrm{V} - \mathrm{Q} = \text{const.}$$

The sum here occurring, which occupies the place of the *vis viva*, and which shall be denoted by L, differs from the ordinary form of this expression by the necessarily positive squares of q_n being not merely multiplied by the necessarily positive inert masses μ_n, but, instead of the latter, the differences $(\mu_n - e_n p_n)$ entering as coefficients of the squares. These differences, however, may become negative, since μ_n can at all events be reduced to what even Weber and C. Neumann regarded as an extraordinarily little inert mass of the electrical quantum e_n, while the quantity p_n, a function formed after the manner of potential functions, may proceed from as great electrical masses as we please. If, now, $e_n p_n > \mu_n$, the point e_n would possess a *quasi* negative mass. *Acceleration of its motion would correspond to a diminution of the vis viva.* If the *vis viva* L consisted of a number of positive and negative terms of this kind, it might conserve an unchanged final value while its negative and its positive terms alike augmented *ad infinitum*.

These relations are represented most simply when only one of the masses μ is supposed to be in motion, and the rest spread over and adherent to a spherical surface surrounding the mass μ (perhaps the surface of an insulator). Then p and P become constants independent of the position of the point μ in the sphere; further, $\mathrm{Q}=0$; and the equation reduces itself to

$$\tfrac{1}{2}(\mu - ep)q^2 + \mathrm{V} = \text{const.}$$

If, now, the quantum of the electricity on the sphere is great enough, so that $ep > \mu$, then q^2 and V must increase and diminish together. If μ moves in a direction *opposite* to the force represented by V, V augments and the velocity q must *increase*. *If, on the contrary, μ moves in the direction of the force, the velocity diminishes.* If μ moves in a prescribed path against a force which constantly resists it (for example, against friction), its velocity must increase continually and *ad infinitum*, with which production of heat *ad infinitum* would be connected. If in its course the mass impinges again and again continually against a greater inert elastic mass, it will drive this onward, and at each impact increase its own velocity, so as to make the next collision more forcible. This would evidently give a *perpetuum mobile*.

It may here be remarked that, if the linear dimensions of the spherical electric layer be increased n-fold, but the density be preserved unaltered, the quantity p will be augmented to n times

as much ; so that we can make it as great as we please, in spite
of continually increasing distance of the acting mass.   We have
here, therefore, by no means to do with actions at molecular
distances, but with distant actions of the Weberian forces.

The case I previously indicated, in which the mass μ attains
infinite velocity, rests on the fact that this must always happen
as often as, under the action of an accelerating force, it arrives
at any place where the coefficient $(\mu - pe)$ representing the mass
becomes $= 0$, because the mass zero receives infinite acceleration
from a finite force.   Besides, in the present memoir, I have
shown that neither is it necessarily at molecular distances that
this takes place, nor does it require an infinite initial velocity, if
only sufficiently large electrical masses are chosen, and if upon
the whole path of the two masses an exterior force acts which
impels them towards each other and is powerful enough to over-
come their electrostatic repulsion.

The objections raised by W. Weber against one of the physi-
cally impossible consequences which, in my earlier memoir, are
deduced from his theory are thus removed.

In his most recent electrodynamic researches, M. C. Neu-
mann has expressed his concurrence in Weber's objections, and,
for his own part, has endeavoured to remove from the theory
the deficiencies pointed out by me, in that he has introduced
an alteration into Weber's expression for very small distances.
From what has just been said it is evident that such an altera-
tion cannot obviate the physical impossibilities mentioned.

Also, for electric currents, no introduction of molecular pro-
cesses, motions, or forces can get rid of the unstable equili-
brium, because when the dimensions are increased n-fold and the
electric densities unchanged the work-equivalent of the mole-
cular processes increases only as n^3, but that of the potentials
as n^4 or n^5, according to whether they proceed from surfaces
or spaces ; so that the latter, if they represent a quantity of
work which is smaller than in the resting equilibrium of elec-
tricity, always obtain the preponderance with a sufficient aug-
mentation.   When everywhere equal quanta of positive and
negative electricity move in opposite directions, the quantities
p_n vanish, but the electrodynamic potential $(-Q)$ may become
less than zero.   That such a distribution of electric densities and
currents may occur has been shown in my earlier memoir, quite
independently of the differential equations which regulate the
course of the currents.

Given a current-distribution which represents a less quantity
of work than that of electric equilibrium, such a flow can only
by the application of exterior work be brought to rest, and
must otherwise, by withdrawal of work, such as takes place

by the development of heat in conductors, be augmented *ad infinitum*.

In this manner an example makes clear how important it is that the analytical expression of the *vis viva* should contain only positive terms; and that this condition is not fulfilled by the action at a distance of Weber's law is here exhibited as the last cause of the physically impossible consequences to which it leads. These, at all events, cannot be removed without very bold new auxiliary hypotheses, which must not only vary the actions at molecular, but also those at greater distances.

In conclusion I have, in the present memoir, endeavoured to clear up the doubts expressed by M. J. Bertrand* respecting the structure of the differential equations of the motion of electricity.

LXIV. *Proceedings of Learned Societies.*

ROYAL SOCIETY.

June 20, 1872.—Sir James Paget, Bart., D.C.L., Vice-President, in the Chair.

THE following communication was read :—
  "On the Spectrum of Nitrogen." By Arthur Schuster, Student at the Physical Laboratory of Owens College.

1. *Introductory.*—The formation of the different spectra which one gas is said to exhibit, when examined under different conditions, still remains one of the most obscure points of spectrum analysis. In 1864, when Plücker and Hittorf published their researches "On the Spectra of Ignited Gases and Vapours, with especial regard to the different Spectra of the same elementary gaseous substance"†, they drew attention to the close resemblance in character of the band-spectra which certain metals yield at a comparatively low temperature to the band-spectrum of nitrogen and sulphur. Roscoe and Clifton, in their paper "On the effect of increased Temperature upon the nature of the Light emitted by the Vapour of certain Metals or Metallic Compounds"‡, rendered it probable that the band-spectra of the metals belonged really to the oxides. The two spectra of nitrogen were not, however, examined from that point of view, but, on the contrary, they were made the starting-point of new investigations by Wüllner, who came to the conclusion that certain gases may give even more than two different spectra. Ångström§, expressing his doubts about the trustworthiness of Wüllner's experiments, says in a note: "As regards the spectra which are usually attributed to nitrogen, I mention here, as a general fact, that it is my conviction that the fluted bands which are so characteristic of the oxides of metals are never found in spectra of elementary gases."

I propose to show, in the present communication, (1) that pure

* *Comptes Rendus de l'Acad. des Sciences,* vol. lxxiii. p. 968.
† Philosophical Transactions, vol. clv. p. 1.
‡ Chemical News, vol. v. p. 233.   § Comptes Rendus, Aug. 1871.

nitrogen gives only one spectrum; (2) that this is the line-spectrum; (3) that the fluted spectrum of the first order is due to oxides of nitrogen, formed under the influence of the electric spark.

2. *First experiment.*—The first experiment which I made with respect to the spectrum of nitrogen, was a repetition of an experiment of Secchi, who found that in different sections of the same tube three different spectra of nitrogen might be obtained. A vacuum-tube was made exactly according to Secchi's description, filled with nitrogen and exhausted. To my astonishment the tube showed, even in its widest parts, only a spectrum of lines. No accurate measurements were taken at the time, but the spectrum was no doubt that of the second order described by Plücker. Suddenly, and while I was looking through the spectroscope, the spectrum changed, and the well-known fluted bands appeared. The first spectrum could now easily be obtained by introducing a Leyden jar in the circuit. The spark very soon ceased to pass, and it was then found that the tube was leaking.

3. The behaviour of this tube at once suggested the idea that the presence of air was necessary for the formation of the fluted spectrum. It is well known that the oxides of nitrogen are formed on passing the electric spark through air, and the resemblance which this spectrum bears to the spectra of the oxides of metals rendered this view probable. In order to test it, a series of experiments were made, showing that,—

(*a*) Whenever the fluted spectrum appeared, it could be shown that traces of oxygen were present ;

(*b*) Whenever there was a certainty of no oxygen being present, the spectrum of the second order appeared under all pressures and in all temperatures.

In order to free the nitrogen from every trace of oxygen, I adopted, at Dr. Stewart's suggestion, the plan of heating a small piece of sodium placed in the vacuum-tube. This proved in each case perfectly satisfactory ; for when every trace of oxygen had thus been absorbed, the line-spectrum alone was invariably obtained[*].

4. *Wave-length of the two spectra.*—There is no possibility of confounding the two spectra. The fluted spectrum is well known by its beautifully shaded violet bands ; but in order to exclude any possibility of error, their position was read off on the reflecting scale of the spectroscope ; the measurements were reduced to wave-lengths, and the following numbers obtained for the least refrangible end of the bands in tenth metres[†]:—

Fluted Spectrum.

5129	4436
4981	4390
4649	4318
4556	4237

[*] The formation of the fluted spectrum does not imply that all the nitrogen in the tube has been oxidized; it has been remarked by different observers, and especially noticed by Plücker, that when the spark passes through a mixture of two gases, the spectrum of one only is often seen.

[†] A tenth metre, according to Ångström, means a metre divided by 10^{10}.

As the measurements were taken merely for the sake of reference, they do not lay claim to great accuracy.

The true spectrum of nitrogen is easily recognized by a very bright green line followed at a small distance towards the more refrangible parts by a green band; it also contains some violet bands, which are not shaded. The position of the principal lines was read off; their wave-lengths, as determined by Dr. Marshall Watts from the measurements made by Plücker, are as follows:—

Line-spectrum.

6243	5767		4214 } band
6176	5666		4199 }
6087	5164	(the green line)	4184 } band
6051	4894		4170 }
5908	4644		

5. *Description of apparatus.*—The tubes generally used had two pockets, A and B, into which small pieces of metallic sodium were introduced by means of the tubes C and D. The tube C was con-

nected with the receiver containing the nitrogen, whilst the tube **D** was connected with the air-pump. The nitrogen was generally prepared by the combustion of phosphorus in air. After a few hours' standing, when all the phosphoric acid formed had been absorbed, the gas became quite clear and was ready for use. This mode of preparation, it is true, does not give the nitrogen very pure; but as my object was to get the nitrogen free from oxygen, and this was easily obtained by means of the absorption by sodium, the method was found sufficient. Other modes of preparing the nitrogen were tried, such as passing air over red-hot copper or the decomposition of ammonia by chlorine, but the same results were invariably obtained. The air-pump used was that of Carré's freezing-machine, with which pressures down to 2 millims. could be easily obtained. When the pressure was measured, a ⊤-shaped tube was employed, one side of which was connected with the Geissler's tube, the other with the pump, while the mercury was drawn up in the longer part of the tube; its height was read off and compared with a barometer. I now pass to the description of the experiments.

6. *Method of experimenting.*—When the air in the vacuum-tube had been exhausted, the communication with the receiver containing the nitrogen was opened, and the gas was allowed to pass through it for some time while the pump was being worked. The tubing connecting the tube with the receiver was then clamped air-tight, and the tube was exhausted.

The electric spark in passing through it exhibited a violet colour, and gave the spectrum of fluted bands:

5129	4436
4981	4390
4649	4318
4556	4237

The sodium was next heated until it presented a clean metallic surface. The light which the tube now emitted was bluish white, and much fainter than before; and the whole appearance of the spectrum had changed to that of the second order with its characteristic green line. It was, however, found that the pressure in the tube had slightly increased, owing most likely to the vapour of the sodium present; and on bringing the mercury to its former level, the spectrum became brighter, but remained the same in character. New nitrogen was then led into the tube, and after exhaustion the old fluted spectrum again appeared; this was, however, at once changed into that of lines by heating the sodium. This process was repeated several times in succession, but invariably with the same result. I have in my possession two tubes sealed off under 2 millims. pressure, one without sodium, showing the fluted bands, the other containing sodium, showing the spectrum of lines. Two other tubes, sealed off under 15 millims. pressure, show the same thing. I have repeatedly convinced myself that, from the highest pressure under which the spark of the induction-coil passes to the lowest pressure which I could obtain with an ordinary air-pump, pure nitrogen invariably gave one and the same line-spectrum. Once, when I intended to seal a tube off under higher pressures, it was found that the sodium was not sufficient to absorb all the oxygen present, so that a sort of mixture of the two spectra was seen. Such a mixture was often observed by Plücker and Wüllner at the point where one spectrum changed into the other; it is characterized by the green line of nitrogen and the fluted violet bands at the same time.

The tube showing the mixture at 15 millims. pressure was gradually exhausted, but the spectrum remained exactly the same. If the formation of the two spectra depends merely upon the pressure or temperature to which the gas is subjected, how can a mixture of the two spectra, indicating a state of transition, exist under so entirely different pressures and different temperature?

In order to ascertain whether nitrogen even carefully prepared contains oxygen, a drop of a solution of iodide of potassium and starch was introduced into the tube; after the spark had passed for a few seconds only, the liquid was coloured blue—showing either the formation of oxides of nitrogen or of ozone, but at any rate the presence of oxygen.

7. *Spectrum of oxides of nitrogen.*—I tried to obtain the spectra of the different oxides of nitrogen; they all give the same fluted spectrum, and I could get no information as to which particular oxide the fluted spectrum is due: this is, however, easily understood if we remember that it is just as difficult to prepare the oxides of nitrogen

free from oxygen as pure nitrogen itself; so that the oxide giving the spectrum in question will always be formed. I have, however, convinced myself that the absorption-bands of nitrous acid gas are not coincident with the bright bands of the spectrum; and it is probable that the spectrum is due to nitric oxide, this being the most stable of all the oxides of nitrogen.

I may add that one of the tubes containing the sodium and showing the lines one day cracked, and then at once showed the violet bands. This fact will not be easily explained by the assumption that the fluted spectrum belongs to a lower pressure and lower temperature than the spectrum of lines.

I propose to subject the different spectra of the remaining gases to a careful examination.

The above experiments were made in the Physical Laboratory of Owens College, Manchester; and I have to thank Professors Balfour Stewart and Roscoe for many valuable suggestions.

GEOLOGICAL SOCIETY.

[Continued from p. 476.]

May 8, 1872.—Joseph Prestwich, Esq., F.R.S., in the Chair.

The following communications were read:—

1. "Notes on Atolls or Lagoon-islands." By S. J. Whitnell, Esq.

The author commenced by indicating certain facts which lead him to think that the areas of atolls are not at present sinking, and referred to one instance (that of Funafuti or Ellice Island) in which he thought there were signs of a slight upward movement. He noticed the occurrence of a furrowed appearance, or a series of ridges or mounds, in some islands, each of which he regarded as produced by a single gale. He also described a freshwater lagoon, about three miles in diameter, as occurring in the island of Quiros.

2. "On the Glacial Phenomena of the Yorkshire Uplands." By J. R. Dakyns, Esq.

The author stated that in Derbyshire and Yorkshire, south of the Aire, there is no glacial drift on the eastern slope of the Pennine chain, except where it is broken through by the valleys of the Wye and of the Aire and Calder. The basin of the Aire and the country northward are thickly covered with drift, which contains no rocks foreign to the basin, and thus points to formation by local action. The author ascribed this to the glaciation of the country in part by glaciers, and in part by a general ice-sheet. Evidence of the latter he finds in the fact that drift occurs only on one side of the valleys—namely, on the lee-side of the hills with respect to the source of the drift materials. Traces of the action of glaciers are:—the great amount of scratched and rounded pebbles in the mounds of drift, which increases in proportion to the distance from their source; the presence of great piles of drift at the junctions of valleys, as if by the shedding of the lateral moraines of two glaciers; and the existence of mounds of pebbles and of an alluvial deposit wherever a rock-basin crosses a

valley. The Kames or Eskers, which are frequent in the valleys, he ascribed to the deposition of moraines in the sea instead of on land.

3. "On a Sea-coast Section of Boulder-clay in Cheshire." By D. Mackintosh, Esq., F.G.S.

The principal object of the author was to draw attention to the fact of the occurrence of numerous sea-shells in a lower boulder-clay at Dawpool as thoroughly *glacial* in its appearance, structure, and composition as any clay to be met with along the shores of the Irish Sea, and differing in no essential respect from the *Pinel*, which runs up the slopes and valleys of the Lake District. He pointed out a number of very important distinctions between the Lower and Upper Boulder-clays of Cheshire, referring especially to the light grey or blue facings of the fractures of the latter. He gave a list of a number of large boulders, greenstone and Criffell granite predominating, though among the smaller stones Silurian grit was most prevalent. The author likewise explained the mode of striation of the stones found in the clay, and the positions they occupied in reference to their flattened surfaces.

The paper was illustrated by samples of the two clays, a number of shells in various states of preservation, and about forty specimens (most of them named and their parentage assigned) of Silurian grit and argillite, greenstone, several varieties of felstone and porphyry, felspathic breccia, Criffell and Eskdale granites, and granites of unknown parentage, Wastdale or Ennesdale syenite, quartz, Carboniferous Limestone, chalk-flints (?), local gypsum, sandstone, &c.

In a letter, Mr. Searles V. Wood, Jun., stated that he regarded the Boulder-clay containing the shells as later than the newest of the East-Anglian beds, and the Upper clay as probably equivalent to the Hessle clay.

The fragmentary shells sent had been determined by Mr. J. Gwyn Jeffreys, who found eleven species represented among them, and stated that they agreed with the shells from Moel-Tryfaen and Macclesfield. He remarked especially on the occurrence of *Astarte borealis*, a species now extinct in the British area.

4. "On Modern Glacial Action in Canada" (second article). By the Rev. William Bleasdell, M.A.

In this paper the author communicated some facts illustrative of the action of ice in Canada, in continuation of a former paper. Fidlar's Island, in the rapids of the river Trent (flowing into the head of Lake Ontario), has been removed within the last eighteen months. Patrick's Island, a mile lower down, is also rapidly disappearing. Salmon Island, in the Bay of Quinte, between Amherst Island and the mainland, which had an area of about an acre fifty years ago, has disappeared, leaving a shoal with about 4 feet of water over it; and three neighbouring islets, known as the Brothers, are in course of removal. The removal of these islands is due to the action of drift-ice. The author also referred to the formation of ground-ice in the Canadian rivers.

May 22, 1872.—Prof. Morris, V.P., in the Chair.

The following communication was read:—

"On the Phosphatic Nodules of the Cretaceous Rock of Cambridgeshire." By the Rev. O. Fisher, M.A., F.G.S.

This paper contained an attempt to explain the origin of the phosphatic nodules which lie in a thin bed at the base of the Chalk in Cambridgeshire and are largely extracted, by washing the stratum, for the purpose of making superphosphate of lime. Two hundred and seventy tons per acre, at the rate of fifty shillings a ton, represents the valuable yield of the deposit, which is followed to the depth of about 18 feet. The nodules and other fossils of the bed are chiefly derivative, forming a concentrated accumulation from a deposit belonging to the Lower Cretaceous period. Some of the fossils, however, are believed to be indigenous to the deposit. *Plicatulæ* are attached to all the derivative fossils and nodules; and the sharp broken surfaces of the latter, with *Plicatulæ* on them, show that they were mineralized before they were deposited in their present *gisement*. The green grains of chlorite have been drifted into patches. Certain calcareous organisms are preserved; but many genera of mollusks only occur as casts in phosphate of lime. The deposition of the phosphatic matter has been determined by *animal* substances. There are two chief varieties of the "ordinary" nodules. The first are amorphous, or else finger-shaped; the second formed like a long cake rolled, partially or wholly, upon a stick. The surface of these two kinds of nodules is coriaceous and wrinkled; and they usually show marks of attachment to some foreign body. Certain species, clearly zoophytes, are converted into phosphatic nodules; and when sections are made of these, they are found to show under the microscope structures and spicula allied to those of Alcyonaria. Slices of the *common* nodules show similar spicula, and occasionally reticular structure. When casts in plaster are made from *Alcyonium digitatum*, and coloured to resemble the nodules, the similarity in general form and structure of surface is very striking. The phosphate was probably segregated by the animal matter from its solution in water charged with carbonic acid, which is a known solvent of the phosphate; an analysis of the matrix has proved that phosphate of lime is appreciably present in it. The author doubted the derivation of the nodules from the denudation of the subjacent Gault, and exhibited a collection of these to show that they were distinguished by more stunted growth.

The deposit was on the whole considered to represent the thin band with similar fossils at the base of the Chloritic Marl, as seen in the west of England, in which district it is underlain by the true arenaceous Greensand. The absence of the true Greensand was attributed to the intervention of the old palæozoic axis of the London area; and it was finally suggested that a similar axis might stretch from Leicestershire to Harwich, causing the change in character of the Lower Cretaceous beds between Cambridgeshire and Norfolk.

LXV. *Intelligence and Miscellaneous Articles.*

ON THE ABSORPTION OF OZONE BY WATER.   BY L. CARUS [*].

OZONE has generally been considered insoluble in water. Meanwhile M. Soret has announced its absorption by that liquid; yet hitherto nothing positive is known on the subject.

It is easy to ascertain that water into which ozonized air or oxygen has been made to pass exhibits all its reactions: it decomposes iodide of potassium, decolours indigo and the sunflower, colours blue the tincture of guaiacum, transforms the protoxides of thallium, manganese, and lead into peroxides; by its action on silver one sometimes even succeeds in determining the formation of the peroxide of that metal. The author has, besides, proved that this water contains neither oxygenated water nor nitrous acid, either free or combined with ammonia, the presence of which might have explained at least a part of these reactions.

The power of absorption of ozone in water cannot be determined with precision, because we can only operate on mixtures in which that gas is only in a very small and never very constant proportion.

In his experiments, the author produced ozone by Soret's method —that is to say, by electrolysis of sulphuric acid spread out and kept at the temperature of zero C. In these conditions the proportion of ozone in the gas, determined by the decomposition of iodide of potassium, was found in two trials to be 0·93 and 1·21 volume per cent., supposing this gas to have a density equal to $\frac{3}{2}$ of that of oxygen.

The gas was caused to pass during from two to three hours into water kept between 2° and 4°; it was then submitted to analysis, and was found to contain per litre, in three experiments:—

0·0109 gramme of ozone, or 5·11 cub. centims.
0·0094   ,,   ,,   4·24   ,,
0·0083   ,,   ,,   3·86   ,,

The author likewise analyzed the ozonized water supplied by the works of MM. Krebs, Kroll & Co., of Berlin, for medical uses. He found in it from 4·06 to 4·45 cubic centims. of ozone per litre; it, too, contained neither oxygenated water, nor nitrous or nitric acid.—*Bibl. Univ., Arch. des Sciences*, vol. xliv. p. 348.

ON THE HEAT OF EXPANSION OF SOLID BODIES.   BY H. BUFF [†].

The augmentation of volume undergone by a solid body by heating is most analogous to the extension produced by the traction of a weight. Moreover one is naturally induced to seek the quantity of the pressure, or of the force of extension, exerted by heat upon the unit of surface. The solution of this question can be found, if we have, besides the coefficient of expansion of a body, its coefficient of traction, both referred to the unit of volume.

Now the coefficient of traction of a certain number of bodies in the direction of their length is known; but the extensibility and com-

[*] *Berichte der deutschen chemischen Gesellschaft*, 1872, p. 520.
[†] Pogg. *Ann.* vol. cxlv. p. 626.

pressibility of the unit of volume have hitherto been but little studied. It is true, however, that Wertheim has demonstrated, or at least has shown it to be very probable, that for homogeneous bodies the coefficients of cubic extension or compression are the same as those of linear extension or compression. He has, moreover, shown that this opinion was confirmed by M. Regnault's experiments on the compressibility of copper, of brass, and of glass.

Probability is also in favour of the accuracy of Wertheim's law. For if the two coefficients of extensibility, linear and cubic, were not the same, this ought also to be the case with the two coefficients of elasticity; and it would follow that the velocity of propagation of sound is not the same in a rod and a ball, both of the same substance and perfectly homogeneous in all directions.

It being, then, admitted that the two coefficients of extensibility are equal, the coefficient of cubic extensibility of iron, for example, referred to the millimetre as unit of length, is $a = 0.0000481$. That is to say, a cube of iron of 1 cub. centim., drawn normal to its six faces by a force of extension of 1 kilom. per square millimetre, has its volume increased 0.0000481 cubic centimetre.

The coefficient of cubic dilatation of iron between $0°$ and $100°$ is, for $1°$ C., $\beta = 0.0000350$. A cubic centim. of iron passing from $0°$ to $1°$ increases therefore 0.0000350 centim. Consequently, to produce the same augmentation of volume as a tension equal to 100 kilogrs. distributed to the six faces, an increase of temperature $\dfrac{a}{\beta} = 1°.374$ would be required.

The mechanical work produced by the heat in this rise of temperature ($1°.374$) of the cube of iron corresponds to the effect necessary to raise 100 kil. 0.000081 gramme-centimetre. Indeed, since each of the three faces of the cube advances during the traction $\dfrac{0.0000481}{3}$ centim., this is equivalent, for the augmentation of volume, to only one of the three faces advancing 0.0000481 centimetre.

The expression of the quantity of heat which a cubic centim. of iron must absorb to be able to produce this work is obtained by multiplying its weight in grammes $\delta = 7.757$ by its specific heat $s = 0.1098$ and by $\dfrac{a}{\beta} = 1.374$. We then take for unit the quantity of heat necessary to raise one gramme of water from $0°$ to $1°$.

The quantity thus obtained, $\delta s \dfrac{a}{\beta} = 1.170$, is the amount of heat necessary in order to give to a mass of iron of 1 cub. centim. a force producing the same expansion as a traction-force of 100 kilogrammes. Only a very small quantity of this heat is used for the dilatation itself, or for the production of the work above estimated, and becomes latent.

If we take in round numbers 42000 grammes as the mechanical effect of the unit of heat adopted, we shall have as the work of 1.17 unit of heat 49140 gramme-centimetres. The effective work of the heat in the dilating cube of iron is equal, as we have seen, to 4.81 gr.-c.

Consequently the quantity of heat employed in the dilatation of the iron is to the total quantity absorbed as 4·81 to 49140, or as 0·98 to 10000.

If we have merely to determine this ratio, the knowledge of which is sufficient for the solution of various questions of theoretical physics, it is not necessary to have more ample data respecting the coefficient of extensibility a, because this vanishes, being a factor common to both the numerator and denominator of that ratio.

We have found $49140 = 42000\, \delta s \dfrac{a}{\beta}$ and $4·81 = 100 \times 1000\, a$,

whence
$$\frac{4·81}{49140} = \frac{\lambda}{10000} = \frac{100000\, a\beta}{42000\, \delta s a} = \frac{\beta}{0·42\, \delta s},$$

and, finally, for the expression of the quantity of heat transformed into work,
$$\lambda = \frac{10000\, \beta}{0·42\, \delta s}.$$

The calculation made for the particular case of iron was performed in the same manner for a few other bodies. The results are recorded in the last column of the following Table. The numbers have sufficient accuracy only for ordinary temperatures.

	$a.$	$\beta.$	$\delta.$	$s.$	$\lambda.$
Iron	0·0000481	0·0000350	7·757	0·1098	0·980
Copper	0·0000951	0·0000515	8·936	0·0949	1·446
Silver	0·0001401	0·0000573	10·301	0·0577	2·378
Gold	0·0001791	0·0000466	18·035	0·0324	1·899
Platinum	0·0000628	0·0000265	21·166	0·0324	0·920
Lead	0·0005634	0·0000854	11·165	0·0314	5·800
Glass	0·0001451	0·0000262	2·446	0·1770	1·441
Water at 16° ..	0·0045854	0·0001600	0·999	1·0000	3·810

The numbers under λ give the quantity of heat become latent, expressed in ten-thousandths of the total heat absorbed. This portion is only a very small aliquot part of the total heat absorbed—and that not only for solid bodies, but also for water. We can thus explain to ourselves why all endeavours to raise the temperature of a solid body by compression have hitherto been vain. We also understand why the latent heat of extension exerts so little influence on the specific heat of the atoms of solid bodies.—*Bibliothèque Universelle, Archives des Sciences Phys. et Nat.* vol. xliv. pp. 341–344.

EXPERIMENTS ON COLLISION WITH BALLS OF DIFFERENT METALS.
BY H. SCHNEEBELI[*].

In a previous communication on the collision of elastic bodies[†] I have investigated the conditions of collision in one and the same substance placed in different conditions. The substance employed was steel as hard as glass, which is elastic to the highest degree. I determined qualitatively in what proportion the duration of the impact depended on the mass, the length, and the height of fall of the striking body.

[*] Pogg. *Ann.* vol. cxlv. p. 328.   [†] See Phil. Mag., Dec. 1872, p. 476.

In the present note I communicate the results I have obtained for collision with balls of different metals.

The method of experimenting was the same as that which I described in my previous memoir. The weight of each of the balls was the same ; and they fell from the same height upon the flat and well-polished face, as hard as glass, of a solid cylinder of steel. The balls not all having the same radius, it would be necessary to make a correction in order to render the conditions equal, since the duration of the impact depends on the curvature of the surface. From the results which have been stated elsewhere, it follows that this correction would be very small (lead and zinc, $2\frac{1}{2}$ per cent.), and that in all cases it may be neglected by the side of the other causes of error presented by these metals when the limit of elasticity is exceeded.

In fact, even with very small heights of fall, such as those which I employed (about 10 millims.), the softest metals undergo a slight permanent deformation, which complicates the result. To render the experiments comparable with each other, I always caused the balls to strike with a fresh portion of their surface.

I commenced by investigating the influence which might be exerted upon the deflection of the magnetized needle by the thermo-electric current produced by the contact of heterogeneous metals at different temperatures. When the ball was put in prolonged contact with the surface of collision connected, like it, with a galvanometer, a slight difference of temperature (such as that resulting from the heating of the hand) sufficed to give the needle an oscillation of 100°. But when the ball only remained on the surface of contact during the shock, there was no perceptible current, even when it rebounded as many as ten times in succession on the plane surface of the steel.

Nevertheless, in order to demonstrate clearly that no thermo-electric current affected the experiments, the ball was heated to about 200° before making it strike. Even in this case there was nothing perceptible at the galvanometer, although the shocks succeeded each other rapidly. The experiments with the heated silver ball were repeated after the insertion of a hydroelectric element in the circuit : it is evident that the duration of the collision was then augmented by the heating of the ball ; but the greater deflection of the needle must not in this case be attributed to a thermoelectric current, but, as we shall see, to a diminution of the elasticity in consequence of the rise of temperature. For these experiments, balls first cast and then turned were employed, of the following metals:—

Ball.	Coefficient of elasticity, E.
Steel	19600
Copper	10500
Zinc	8700
Brass	8540
Silver	7140
Tin....	4000
Lead	1700

I transcribe here only two of the very numerous series of experiments I have made, reminding the reader that all the balls fell from the same height upon the plane surface of steel.

	Deviation at the Galvanometer.	
Ball.	1st series.	2nd series.
Steel	72·5	84·2
Copper	94·2	115·0
Zinc	111·0	130·0
Brass	110·5	127
Silver	112	130
Tin	164	194
Lead	270	320

The first series were performed on the 11th, and the second on the 22nd of January.

As general result we may deduce from these two series that the duration of collision increases when the coefficient of elasticity diminishes. A closer connexion appears between the duration of collision and the coefficient of elasticity when we form the product of that duration and the square root of the coefficient of elasticity of each ball. In this way we obtain the following Table:—

Ball.	Series 1.				Series II.		
	$\sqrt{\mathrm{E}}$.	a.	$a\sqrt{\mathrm{E}}$	Corr.	a.	$a\sqrt{\mathrm{E}}$	Corr.
Steel	140	·725	101·5	+ 0·5	·842	117·9	+ 2·1
Copper	102	·942	96·1	+ 5·9	1·150	117·3	+ 2·7
Zinc	93·3	1·110	103·6	− 1·6	1·30	121·3	− 1·3
Brass	92·4	1·105	102·1	+ 0·1	1·27	117·4	+ 2·6
Silver	84·5	1·12	94·6	+ 7·4	1·30	110·0	+10
Tin	63	1·64	103	− 1	1·94	122	− 2
Lead	42	2·70	113	−11	3·20	134	−14
	Mean... 102·0				Mean... 120·0		

From this Table we are justified in concluding that, *when a series of elastic bodies strike the same elastic surface, the durations of the impacts are inversely proportional to the square roots of their coefficients of elasticity.*

All the metals operated on conform to this law with sufficient approximation, except the ball of silver and that of lead. With respect to the latter, this ought not to surprise us; for the elasticity of lead is so slight that even with our small heights of fall the limit of elasticity is considerably exceeded. As to the silver ball, we must suppose that its coefficient of elasticity is sensibly greater than that of the substance on which the measurement of the coefficient has been made. This supposition is by no means inadmissible when we consider the very notable divergences on this point between the results of different observers.—*Bibliothèque Universelle, Archives des Sciences Phys. et Nat.* vol. xliv. pp. 335–338.

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END OF THE FORTY-FOURTH VOLUME.

ALERE   FLAMMAM.